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ARTICLES

American Political Science Review Vol. 88, No. 4 December 1994

DIVIDED GOVERNMENT, FISCAL INSTITUTIONS, AND BUDGET DEFICITS: EVIDENCE FROM THE STATES

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Does partisan control of American state government have systematic effects on state spending and taxing levels? Does divided control affect the government's ability to make hard decisions? Do institutional rules like legal deficit carryover restrictions matter? Using a formal model of fiscal policy to guide empirical analysis of data covering the American states from 1968 to 1987, we conclude that (1) aggregate state budget totals are driven by different factors under Democrats and Republicans, the net result being that Democrats target spending (and taxes) to higher shares of state-level personal income; (2) divided government is less able to react to revenue shocks that lead to budget deficits, particularly where different parties control each chamber of the legislature; and (3) unified party governments with restricted ability to carry deficits into the next fiscal year (outside the South) have sharper reactions to negative revenue shocks than those without restrictions.

Using two decades' of evidence from the American states, we argue that divided government has fiscal consequences and that variations in institutions and rules affect fiscal outcomes and timing as well. Deficits at the state level thus have both economic and political antecedents. In a nutshell, big state deficits arise in recessions, but we find they do so where different parties control each legislative chamber or where one party controls all branches of the state government and the state constitution permits deficits to be carried over from one fiscal year to the next. We show how state constitutions and patterns of party control interact with the division of powers between an executive and a bicameral legislature to produce these results, which are consistent with some from other political contexts.

Our study draws on and has implications for three separate literatures. One is the analysis of demand, institutions, laws, federalism, and process on fiscal policy in the states,[1] which has recently become prominent as American state governments have experienced significant fiscal difficulties after a long period of relative prosperity. Governors and legislatures have engaged in bitter battles over spending and taxes in attempts to pass balanced budgets. The prolonged fiscal crisis in Massachusetts and more recently the turmoil in California captured headlines, but in fact problems are widespread. For example, 10 states began their 1992 fiscal year without a budget, including New York, which set a record by going over three months past its April 1 deadline before adopting a budget, and Maine and Connecticut, which temporarily shut down all but essential public services. Some observers blame this pattern on excessive spending by state governments in the 1980s, while others argue that the "new federalism" is to blame, by increasing state responsibilities while decreasing federal revenue contributions.

Our casual observation that 9 of the 10 states lacking a budget at the beginning of the 1992 fiscal year had divided government[2] brought up another literature that links divided government with budgetary outcomes and in particular deficits and debt. Some question whether governments under divided control can make hard choices and respond forcefully to crises (Cutler 1989; Sundquist 1988) or coordinate to raise taxes (Roubini and Sachs 1989). Others attribute recent U.S. federal deficits to divided party control of Congress (McCubbins 1991) or the lack of presidential party leadership (Inman and Fitts 1990).

Finally, we build on the political–economic "partisan model" of elected officials from competing parties whose core or typical constituents have different preferences which can be addressed through public provision and whose reelection depends at least partly on how well they serve their constituents' wishes. This model has been applied to economic outcomes (e.g., Alesina and Sachs 1990), monetary policy (Alt 1991; Chappell and Keech 1986; Hibbs 1987), American federal expenditures (Kiewiet and McCubbins 1991; Su, Kamlet, and Mowery 1993), and receipts (Cox and McCubbins 1992) and to other countries. Yet attempts to find partisan effects on spending at the state level have had little success "even after the imposition of rigorous controls" (Erikson, Wright, and McIver 1989, 730). We use a somewhat different specification from theirs and find limited but explicable party differences in state-level taxing and spending.

American state governments provide a natural laboratory for studying these relationships between partisan control of government, different institutional constraints, and policy outcomes. In each year from 1968 through 1987, at least 18 of the 48 continental states had divided party control of their governments. Outside the southern and border states, unified party control of both the executive and legislative branches existed less than half of the time and split-legislature governments (where each party controls one house) occurred almost a fifth of the time. The states generally replicate federal institutions (e.g., all states but Nebraska have bicameral legislatures), but they vary significantly in their institutional details of budgeting and deficits. Every state but Vermont has some formal constraint on fiscal discretion, including requirements for governors to submit (and/or for legislatures to pass) a balanced budget, restrictions on rolling over the deficit across fiscal years (some states having biennial budget cycles), and line-item vetoes for governors. Many states also have restrictions on their ability to issue debt, and some states have recently acquired further restrictions on their ability to increase their sources of revenue (Advisory Commission on Intergovernmental Relations [ACIR] 1989; Council of State Governments 1990). Finally, states have less fiscal autonomy than does the federal government, because of federally mandated programs that are less than fully funded (Chubb 1985).

Our model of fiscal policy in the states takes each of these features into account. Starting from a popular tax-smoothing model of optimal deficits, we explicitly recognize the role of bargaining among elected politicians with different partisan preferences in formulating fiscal policy, particularly in reacting to unforeseen shocks such as a drop in revenue. We model how different partisan configurations will respond to revenue shocks that cause the government's fiscal-year surplus or deficit to deviate from expectations. We then empirically test hypotheses derived from this model by using pooled time-series cross-sectional data to estimate simultaneous equations for the determination of revenues and expenditures and by running simulations of fiscal policy outcomes under different partisan configurations for states with and without limitations on their ability to carry deficits over time.

We find three general results. First, divided party control matters—particularly in the response to exogenous shocks. Governments characterized by divided control of the legislature find it particularly difficult to adjust to exogenous shocks by returning the fiscal balance to its previous path within a relatively short time. Second, institutional constraints on the ability of state governments to manage fiscal policy matter—at least where party control of government is unified. Unified governments in states that are not permitted to carry a deficit over into the next fiscal year react quickly to income shocks so that there is little deviation in the time path of surpluses or deficits. In states without such constraints, governments are less likely to react quickly, and thus more apt to suffer deficits as a result of changes in the economy and fiscal federalism. Third, systematic partisan differences do exist between Democrats and Republicans, though the data indeed do not reveal as simple a picture as Democrats just taxing and spending more (see Garand 1988). Rather, the parties have different goals and react differently to changes in permanent expected levels of income, in ways affected by federal aid and the business cycle. Thus the level of spending at any particular year depends on partisan preferences, past histories of spending and party control, and these other exogenous variables.

A POLITICO-ECONOMIC MODEL OF FISCAL POLICY IN THE STATES

To create politico-economic foundations for our analysis, we extend Barro's (1979, 1986) tax-smoothing model of optimal fiscal policy to incorporate parties and divided constitutional government. We then offer a stylized description of the budget process given different partisan preferences, provide a model of reactions to revenue shocks by different partisan configurations (subject to legal constraints), and derive hypotheses that can be subjected to empirical tests.

From Benevolent Dictators to Divided Party Government

In the model of optimal deficits, policymakers can distinguish permanent from transitory demand for spending, and also know when it's a good time to borrow. Governments set taxes to minimize deadweight loss from now until eternity, given some target level of government spending. In this model of a benevolent despot maximizing the welfare of a representative, ageless consumer, budgets are made in which new borrowing is smooth over time, save only for unexpected shocks, transitory changes, and the relationship between interest rates and expected prices. Optimally, taxes are held constant unless there is a change in permanent expected income, with deficits occurring during times of temporarily high spending, such as wars or recessions.

First we introduce parties into the model. Now policy is set by politicians maximizing some objective function based on their own constituencies' (parties') preferences, rather than the unweighted sum of consumer and producer surplus. In the simplest case there are two sorts of policymakers, each of whom has an economically distinct and different core of supporters whose interests are maximized. One group represents constituents who favor public spending of a higher share of permanent income (*high demand*) and the other a lower share (*low demand*).[3] Everything else in the economic model still holds, but the *permanent share* of income that government tends to spend oscillates up and down as the parties alternate in power.

We next take account of institutions that divide control over budgeting between a (bicameral) legislature with proposal power and a governor with veto power. Strictly speaking, if all the branches and houses are controlled by the same party (and there is full information, no factions, and no timing problems), nothing is added to the policy model. But there are other possibilities. One party could control each branch, or one party could control the governorship and only one house. Either information or coordination problems could emerge in these different partisan configurations. Particularly in the partisan divided-legislature case, when the two houses have to bargain with each other (possibly under time pressure) to reach a mutually agreeable proposal and have bilateral veto powers, each (at a cost) can stop the other from getting anything. Now a lot of time could be spent off the "optimal" equilibrium path.

Finally, constitutions have effects. Laws exist in some cases that specifically prevent increased borrowing to make up for an unexpected revenue shortfall. Then even a Barro-type government cannot borrow just to offset a transitory fluctuation, let alone a partisan government to shift upward the share of permanent income spent.

A Stylized Budget Process and the Response to Shocks of Partisan Configurations

Given this foundation, we next stylize the process of budget formation in order to derive specific testable predictions about fiscal policy in these varying partisan, institutional, and constitutional contexts.[4] We define *the budget* as expected levels of spending and revenue during the coming fiscal year as determined by elected officials organized into political parties. These officials attempt to finance their partisans' desired or target level of expenditure, given expected levels of income, net of any purely transitory shocks or unplanned surpluses or deficits. Whether financing involves borrowing or only revenues depends at least partly on constitutional provisions, as will be discussed. The budget process is that the executive submits an initial proposal, but only the legislature can initiate a change in either spending or revenues. The executive can either approve or veto proposed changes, but generally must exercise the veto power within a certain time period after the legislature has acted. If the legislature fails to make a proposal or if a proposal is vetoed and the veto is sustained, then we assume that the budget is set equal to its reversion level, which is the previous year's expected levels plus any persistent effects of the unforeseen shock.[5]

We focus throughout on the "steady-state" behavior of different partisan configurations, holding for future work detailed analyses of initial transitions and any preelection end game.[6] We ask how different configurations of party control affect the way governments behave and particularly how they react to unplanned surpluses or deficits (which we believe usually arise as unforeseen revenue shocks)[7] in setting revenue and spending targets for the next fiscal year. To formulate an empirical model, we initially assume and subsequently attempt to verify empirically

HYPOTHESIS 1. *In the states, the Democrats are the high-demand party, and the Republicans the low-demand party, and this difference is reflected empirically in their* desired *spending levels.*[8]

However, in a government consisting of an executive and a bicameral legislature for which two political parties compete, there are eight possible combinations of partisan control (Fiorina 1992). These divide into three primary forms: unified party government (one party controls all branches), split-legislature government (one party controls each chamber), and split-branch government (the same party controls both chambers of the legislature, but the other party holds the governorship).

In the model we next lay out, the asymmetric roles of the executive and legislature in making fiscal policy interact with these patterns of control to produce important differences in reactions to unplanned surpluses. Unified governments prefer to react by increasing expected revenues following a deficit and decreasing them following a surplus, whereas split-branch governments may seek to adjust both spending and revenues to different degrees, depending on the particular circumstances. In contrast, split legislature governments may not be able to agree on any reaction. In all, we derive seven further hypotheses, dealing with both the composition and the timing of adjustment. We stress that our stylization is sufficient rather than necessary to give rise to these and other empirical patterns we discuss.

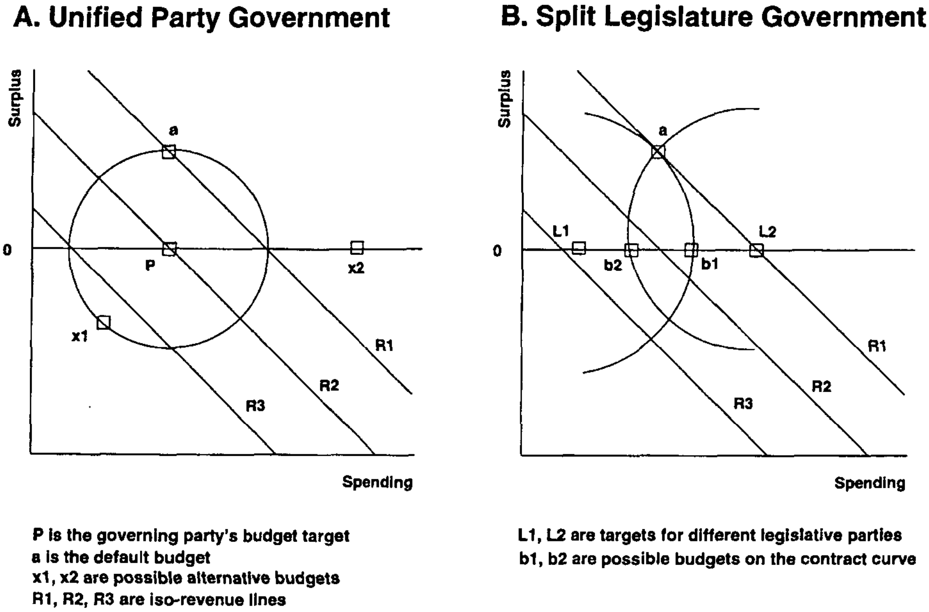
Unified Party Government. Figure 1(a) shows a two-dimensional policy space where the horizontal axis is the level of total spending and the vertical axis is the budget surplus/deficit. For simplicity, the diagram is drawn as if parties prefer a balanced budget. Of course, it may be that parties actually prefer a deficit or a slight surplus (in order to create a "rainy day" fund), and they might even disagree over the appropriate amount of the surplus. In our analysis, however, this would not affect the basic dynamics of reactions to unforeseen shocks.

Since the surplus is identically equal to revenue minus spending, there is a unique level of revenue associated with each combination of surplus and spending. The parallel diagonal lines labeled R1, R2, and R3 represent three iso-revenue lines, with the difference in revenue levels being equal to the vertical distance between two lines. In other words, for a given level of spending, the difference in revenue between R1 and R2 is the difference in the surplus caused by each revenue level. The point labeled P is the bliss point for the party in power, assuming revenue R2, with the surrounding circle being an indifference contour.[9] This mapping implies that a spending level close to the bliss point but creating a deficit, such as at point x_1, is preferred to a spending level farther away but with a balanced budget, such as at x_2. There is no particular requirement that indifference curves be circular, only that they are quasi concave in each quadrant with respect to the bliss point, so that no point can be preferred to another point that is both closer to a balanced budget and closer to the target spending level.

In the steady state, if one party has complete control, it acts as though it will govern forever. Thus with income at its permanent expected level the budget is simply set at point P. Following a revenue

FIGURE 1

Ideal Points and Responses to Shocks of Unified and Split Legislature Governments.


A. Unified Party Government
Surplus
0
a
P
x1
x2
R1
R2
R3
Spending
P is the governing party's budget target
a is the default budget
x1, x2 are possible alternative budgets
R1, R2, R3 are iso-revenue lines
B. Split Legislature Government
Surplus
0
a
L1
L2
b2
b1
R1
R2
R3
Spending
L1, L2 are targets for different legislative parties
b1, b2 are possible budgets on the contract curve

shock, the default budget in the case of inaction such as point a lies on the vertical line passing through P. Because point P is preferred to every other point on this line, there is no trade-off: the government adjusts revenues to restore the budget to point P. If the shock is entirely transitory, then the reversion budget is at point P, so the government optimally increases or reduces debt as far as the law allows:[10]

Hypothesis 2. *Holding other exogenous variables at permanent expected levels, expected revenues will be adjusted downward in the year following a surplus and upward following a deficit; but so long as the adjustment is to a revenue shock, a unified government will not significantly adjust spending, no matter which party is in control.*

Divided Legislatures. If parties differ in their preferred levels of spending, one a high-demand, and the other a low-demand party, then the expected spending level for any given year cannot be optimal for both parties and will probably not be the same as the optimal level that would be chosen by either party in unified government. The key difference between split-legislature government and unified (or indeed split-branch) government is that if each party controls one chamber of the legislature, each possesses a veto over the other and the set of possible agreements contains multiple points determined by the indifference curves passing through the default budget.[11] We show this in Figure 1(b), where point L_1 represents the ideal point of the low-demand party, point L_2 represents the ideal point of the high-demand party, point a represents the default budget, and the contract curve is the portion of the balanced budget line connecting points b_1 and b_2.

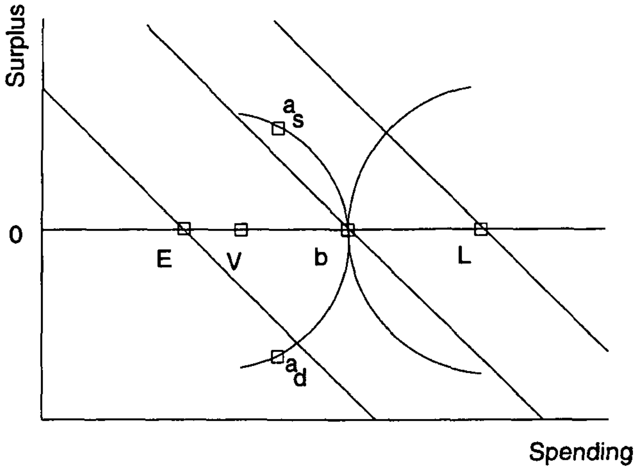
Because there is no unique equilibrium, each party will be tempted to hold out in order to seek a better deal. In Figure 1(b), for example, party L_1 may be able to achieve point b_2 if it can outlast party L_2 but will have to settle for point b_1 otherwise. This expected bargaining could produce any outcome lying between b_1 and b_2:

Hypothesis 3. *Split legislatures adjust to revenue shocks with a mix of tax and (even possibly preponderantly) spending changes, unlike unified governments.*

Split legislatures can respond to shocks, but this is nevertheless the likeliest situation for significant delay to occur. In such situations, as in the model of Alesina and Drazen (1991), both parties choose an optimal time to concede based on their own cost of delay, the advantage gained by waiting for the other party to concede first, and their expectation that the other party will do so. Such delay imposes costs on both parties' constituents, in the form of distortionary deficit financing or undistributed surpluses. Balanced against this is the possibility that the party that holds out longest may be able to secure a better result. The politicians risk voter anger for failure to act, but the political costs of delay will be lower in the case of divided government, since voters are uncertain which party to blame. Given this trade-off of costs and benefits, both parties will likely choose a strategy calling for some positive delay, and the result may be a "war of attrition," with no agreement during the current fiscal year. Some uncertainty regarding the other party's payoffs is crucial: if each party has perfect information, then each knows who will have to concede first, so the eventual losing party might as well avoid the cost of delay. In our case, especially

FIGURE 2

Ideal Points and Responses to Shocks of Split Branch Government with a High Demand Legislature.



L is the legislative party's budget target
E is the executive party's budget target
V is the target of the last vote needed to override

a_s, a_d are default budgets
b is the budget adopted
Diagonal lines are iso-revenue lines

when legislatures are newly split, each party is likely to be uncertain of the other's willingness to risk the political consequences of failure to make an adjustment:[12]

HYPOTHESIS 4. *Delayed responses, or even the absence of systematic responses to shocks (producing larger cumulative imbalances as reversion levels persist) are likelier under split legislatures than unified government.*

Split-Branch Government. In this form of divided government, one party controls the executive, and the other controls both chambers of the legislature. Figure 2 illustrates how adjustments to expected spending or revenues depend on (1) whether the high-demand party controls the executive or the legislature, (2) whether the previous year ended in a surplus or a deficit, and (3) the location and preferences of the last legislative vote needed to override an executive veto, which we assume for now is "all-or-nothing." Ideal points for the median legislator and the executive are labeled L and E, respectively, while the point labeled V is the ideal point of the last legislator required to override an executive veto. Note that if parties are perfectly homogeneous, V will be the same as either L or E, depending on whether the legislative majority party controls sufficient votes to override a veto.[13] The following discussion based on Ferejohn and Shipan (1990), focuses on the more general case of heterogeneous parties. As before, let point a represent the default budget in the case of no new agreement. Figure 2 shows the case where the "high-demand" party controls the legislature while the "low-demand" party controls the executive. A right-to-left mirror image of Figure 2 would illustrate the case where party control is reversed. Testable predictions about budget adjustment follow in each case.

The legislature's strategic advantage from being the only branch that can make proposals (plus our assumptions regarding preferences and the government's ability to implement the equilibrium solution) means that on the whole, the legislature can take advantage of a revenue shock to shift spending in the direction of its bliss point. For convenience, let us focus first on the case of an unexpected surplus, designated a_s. In general, a new budget will be set at the point of tangency between L's indifference curves and the indifference curve for V that passes through point a_s. This point is labeled b in Figure 2. In other words, the legislative party will be able to push through a budget that maximizes the utility of the median legislator, contingent on leaving the last legislator needed to override a veto indifferent between the new budget and the default budget that would prevail if a veto were sustained. In equilibrium, the legislature will propose this budget immediately, and the executive will accept because a veto will not be sustained.[14] By comparing the location of points a_s and b, we see that the expected reaction to a positive unplanned surplus is an increase in spending but that expected revenue does not change. By shifting the location of point V and the shape of the indifference curves, expected revenues could decrease—or even increase—in response to a surplus. So long as indifference curves are quasi concave with respect to the bliss point, however, it is impossible for expected spending to decrease. Moreover, on the whole the direction of the effect of a revenue shock on spending and revenue targets does not depend on whether V is located between E and L (Alt and Lowry 1993).

In the other case in Figure 2, a high-demand legislature responds to the opportunity created by an unplanned deficit (point a_d) by increasing expected spending and then increasing expected revenues by enough more to balance the budget. We can use the same reasoning to the mirror-image cases to show that a low-demand legislature reacts to an unplanned surplus by decreasing both spending and revenues, while a low-demand legislature reacts to a deficit by decreasing spending. Our fifth hypothesis specifies these four subcases, which predict—for any given reversion level, conditional on the direction of the shock—whether spending increases or decreases, and in some cases specify the direction of change in revenues as well, all depending on the preferences of the critical veto vote:

HYPOTHESIS 5. *The response of split-branch government to an unexpected shock is this specified mix of tax-and-spending changes, again unlike the case of unified government.*

But unlike the split-legislature case, in general the executive can either approve or veto changes proposed by the legislature but must exercise the veto power within a certain time period (usually limited to a number of days) after the legislature has acted, or the legislative proposal takes effect. This asymmetric power with limited time for delay is the main reason behind

HYPOTHESIS 6. *Split branch government (like unified government) does* not *result in delay as readily as split legislatures, where there is either no explicit time limit or a much longer one within which to make a proposal.*

Balanced Budget and Deficit Carryover Laws. Our model does not directly address incentives to borrow nor how these might vary across the public, parties, or partisan governing configurations. This would be beyond our scope here. However, the states also have a variety of balanced-budget laws that might influence fiscal policy, though the existing literature is sketchy and inconclusive (e.g., Abrams and Dougan 1986; Bunche 1991). These laws are of two fundamentally different kinds, some requiring balance between projected revenues at the beginning of the fiscal year and others prohibiting ending a fiscal year in deficit or carrying over such a deficit into a new year. The first kind work (if they do work) by focusing attention on the credibility of revenue forecasts and spending plans. In the second case, since the end of one fiscal year coincides with the process of creating the budget for the next, the observation of an imbalance does not just bring forecasts into question but is an action-forcing event: indeed, some of the laws explicitly nullify unfunded expenditures.

Where no such law exists, a government can respond to a surprise deficit by borrowing. Indeed, if the shock is transitory, it is optimal to do so. Where such laws do exist, the response must be to raise taxes, cut spending, or disobey the law (e.g., use accounting tricks to hide the shortfall). All of these, we assume, are undesirable from the government's point of view. Therefore we believe that such laws if enforced must discourage borrowing and excess spending. Even if there is no direct enforcement (e.g., no possibility of legal redress), we believe that if the laws' provisions are sufficiently transparent, they provide a natural focus for interest groups, media, and candidates of other parties seeking to mobilize public opinion against excess borrowing. While borrowing can be hidden in the short run, where deficits cumulate they are more likely to be discovered:

HYPOTHESIS 7. *Other things equal, where a deficit carryover prohibition exists, delayed response to a negative revenue shock resulting in large cumulative deficits is less likely.*

However, if the accountability of government just described is indeed the enforcement mechanism, then where all the parties participate in the government, there is no single culpable party to vote out of office:

HYPOTHESIS 8. *Other things equal, where a deficit carryover prohibition exists, delayed response to a negative revenue shock resulting in large cumulative deficits is less likely under unified than divided—especially split-legislature—government.*

DATA AND ESTIMATION OF REVENUES, EXPENDITURES, AND SURPLUSES

To test these hypotheses, we employ an empirical model of state fiscal policy. The model links revenues and expenditures in the annual budget process with an allowance for the role of a federal system of grants and matching funds, the possibility of adjustments to past deficits and surpluses, and the desires of (and constraints on) different configurations of party control of government. We review our data and model, then present and discuss the results.

Economic and Fiscal Variables

The data for revenues, spending, federal contributions, personal income, and unemployment cover the 48 states other than Alaska and Hawaii from (fiscal) 1968 through 1987.[15] All variables (except the unemployment rate) are in real 1982–84 per capita dollar figures, calculated using the regional CPI–U price index published by the Bureau of Labor Statistics. Unemployment averaged just over 6%, ranging from under 2% in New Hampshire in the early years to 15–18% in West Virginia during the recession of the early 1980s.[16] Average income per capita for the whole period was just under $11,000 (but was 10% lower in the South) and grew on average by 1.35% per annum.

On average, state spending *as a share of income* increased from 9.7% in 1968 to 13.1% in 1976, then decreased to about 12% in 1978, after which it remained roughly constant. Aggregate, positive budget surpluses existed in every year except 1971 and 1975 and increased every year in both real per capita and percentage terms from 1981 through 1987, during which time they averaged just over 1% of per capita income per year. Spending and revenues are higher (by over 1% of personal income) in states with unified Democratic as opposed to Republican government, and aggregate surpluses are higher as well. These differences are not due to period effects, and they are also not statistically significant. Moreover, there is a lot of state-to-state variation. For instance, across the whole period Massachusetts expenditures actually exceeded aggregate revenues (and the overall balance was barely positive in Connecticut and Vermont) while there were large cumulative surpluses (over 14% of total expenditures) in Wyoming, New Mexico, and Nevada. Finally, federal contributions as a percentage of income increased (on average) from 2.2% in 1968 to 3% in 1976–77, then dropped to about 2.5% in the Reagan years. Federal contributions typically were 20–25% of total state spending, though the early period saw extremes of 13% in Delaware and 40% in Wyoming.

Political Variables

Testing our hypotheses requires that we distinguish among states with unified Democratic, unified Republican, split-branch, and split-legislature government. In each year from 1968 through 1987, between 18 and 25 of the 48 continental states had some form of divided party control. On average, outside the 15 southern and border states, unified control of both the executive and legislative branches existed less than 45% of the time, split-branch governments approximately 37%, and split-legislature governments 18%.[17] In the South, there is unified Democratic government over three-fourths of the time, and split-branch (Republican governors) the rest.

Nor is each of these partisan configurations typical of only a handful of states. All 32 nonsouthern states we study experienced unified government: 22 had some unified Democratic government, 24 had some unified Republican government, and 13 had some of both in our data period. Twenty-six of the 32 had a split legislature at one time or another, while 30 experienced split-branch government. Put another way, 11 of these 32 states experienced all four of these types of partisan control, 15 experienced three of the four, and only six experienced as few as 2 types![18] Nor was any partisan configuration particularly ephemeral. The 22 states having unified Democratic government had 6.8 years of it on average, while the corresponding averages are 5.4 years for the states having unified Republican government, 6.1 years for split-branch, and 4.3 years for split legislatures. So each analysis of a partisan configuration is actually based on data from most of the states and for several years in the case of each state.

A vital political variable in our study is whether the state has some sort of end-of-year budget imbalance prohibition. According to the Advisory Commission on Intergovernmental Relations (ACIR), 26 of the states in our sample are actually *prohibited* from carrying over a deficit into the next fiscal year (or biennium) (1989, tbl. 73).[19] Though the ACIR does not provide a precise definition, state constitutions we have examined suggest that such laws include provisions nullifying appropriations or expenditures in excess of revenues for that fiscal year (see Alabama Constitution, amend. 26), although many states allow an exception (i.e., borrowing) if the legislature increases taxes by an amount sufficient to cover the additional expenditure within a specified time period (see Georgia Constitution, 7.4.1). To the best of our knowledge, none of the states having such laws changed them in our data period, though we note that other classificatory schemes also exist. Our attempts to measure the enforcement of these laws directly have so far proved unavailing.[20]

Model, Estimation, and Numerical Analysis

Transitional and transitory influences apart, partisan state governments set expected revenues equal to the share of permanent income (including federal contributions) their supporters favor spending. In practice, they do this by updating last year's revenues (which, under continuing unified government, were expected to be at last year's ideal point) for changes in aggregate personal income and federal revenue contributions and by making any adjustments in light of the previous year's (unexpected) surplus or deficit. Expected expenditures are then set equal to expected available revenue except for any allowances made for transitory influences of the business cycle (represented by the unemployment rate) and a possible further adjustment in response to the previous surplus or deficit. This formulation allows income and federal contributions to affect expenditures indirectly through their effects on revenues. This produces the following structural model:

$$R_t = a_1 + a_2{}^*R_{t-1} + a_3{}^*Y_t + a_4{}^*F_t + a_5{}^*S_{t-1} + v_t \tag{1}$$

$$E_t = b_1 + b_2{}^*R_t + b_3{}^*U_t + b_4{}^*S_{t-1} + w_t, \tag{2}$$

where R = total state revenues, E = total state expenditures, S = state surplus (R − E), Y = state personal income, F = federal government contributions, U = state unemployment rate, and v and w are normally distributed residuals with mean zero.[21]

We estimate the parameters of equations 1 and 2 simultaneously, using a three-stage least squares procedure, treating the state-level income and unemployment and federal contributions as exogenous. The inclusion of lagged revenues in equation 1 appears to eliminate any need for further correction for serial correlation, besides picking up most of the fixed effects associated with different observations in our time-series cross-section data. We have undertaken considerable experimentation with other estimation procedures and specifications and believe the results we report are robust.

However, the theory that we wish to test also involves how different partisan configurations adjust revenues and expenditures in response to the surplus or deficit itself. For example, our second hypothesis was that unified governments adjust revenues but not expenditures in response to a surplus or deficit. In the case of revenues, the hypothesis is simply that $a_5 < 0$ (taxes should be reduced after a surplus). In the revenue case, hypothesis 3 predicts that under split legislatures $a_5 = 0$ (or at least is smaller in magnitude than in the case of unified government).

To find the total effect of the surplus/deficit on expenditures, we need to obtain the reduced-form expenditure function by substituting for revenues in the expenditure equation and rearranging terms (shown in detail in Appendix A):

$$E_t = (b_1 + b_2a_1) + b_2a_2{}^*E_{t-1} + b_2a_3{}^*Y_t + b_2a_4{}^*F_t + b_3{}^*U_t + (b_2(a_2 + a_5) + b_4){}^*S_{t-1}. \quad \textbf{(3)}$$

Note that the full effects of income and federal grants on expenditures involve two parameters and that the total effect of the lagged surplus on expenditures is actually a combination of four parameters taken from the structural equations. These combinations form the basis of testing several of our hypotheses. For example, our second hypothesis was that unified governments would not adjust expenditures in response to a surprise imbalance. In terms of the estimates, this hypothesis is thus that for unified governments $(b_2(a_2 + a_5) + b_4) = 0$.

Unfortunately, some other hypotheses cannot be formalized so simply and require that we turn to simulation methods or numerical analysis. Evaluating different adjustment speeds or propensities to run cumulative deficits requires examining the behavior of the whole model over time, seeing how the dynamic parameter estimates interact with growth rates and repeated stochastic shocks to produce longer-term time paths for revenues, expenditures, and the surplus/deficit. We subject models reflecting the parameter estimates for different combinations of partisan control and legal restrictions to different starting values of income, in order to produce initial revenue shocks. We then examine the outcomes of many such runs, each differing from the others of its kind only by random elements included to represent the uncertainty of our statistical estimates. Since we fix the other initial conditions and exogenous variables at the appropriate subgroup average levels, we can isolate the effects of a recession, a split legislature, a balanced-budget law, or a combination of these on the average deficit after several years. The precise methodology is laid out in Appendix B.

EMPIRICAL RESULTS

We present the results in three parts. First, we provide the regression results and defend our choice of specification. To avoid bias, we estimate the structure directly. Second, given our model with an endogenous variable (revenue) in the expenditure equation, we combine structural and reduced-form estimates to test hypotheses about spending and the composition of response to shocks. Third, to get at the longer-term implications of the dynamic aspects of the model, we examine simulation results to investigate the conditions under which recessions produce larger cumulative deficits. We draw out some further implications in the conclusion.

Regression Results

Table 1 shows parameter estimates for the structural equations. It contains estimates for the nine parameters of equations 1 and 2 separated into eight mutually exclusive cases by region, party configuration, and constitution. The first four subsets are nonsouthern unified governments divided by party and by whether deficit carry over is restricted. (Each subset contains at least 10 different states.) Then there are the nonsouthern split-branch and split-legislature cases, as well as the two party configurations present in the South. Estimates of some state-level fixed effects are omitted from the table in the interests of space.[22]

The coefficient estimates reveal similarities and differences across cases. In the revenue equations, the estimated coefficient of lagged revenue (a_2) is generally in the region of .85 to .9 and has a small standard error—so revenues are quite sticky, tracking previous year's values closely. The income coefficient (a_3) varies from .008 to .037. Reading across the first four columns, this means that an extra thousand dollars per capita income in a given state–year produces from \$12 to \$16 more revenue in Republican states and from \$22 to \$37 more revenue in nonsouthern Democratic states, consistent with hypothesis 1 identifying the Democrats as the high- and Republicans as the low-demand parties. We include the insignificant coefficients differentiating split-control cases by governor's party in columns 5 and 6 to show that party differences in those cases are not captured by such a simple coding. Direct dependence of revenues on federal contributions is significantly estimated in all cases but one, but the extent of this dependence varies considerably from case to case.

The response of revenues to a surplus confirms (partly) the stabilization implications of the model as outlined in hypotheses 2 and 3; that is, the largest and most significant negative coefficients of the lagged surplus (a_5) in the revenue equation appear in the cases for nonsouthern unified governments (except for Democrats in legally restricted contexts, where the coefficient cannot be distinguished from zero). The estimates are negative and significant but at a lower absolute value for nonsouthern split-branch governments (and in the South) and insignificant for split legislatures. This is what we predicted and illustrated in Figures 1 and 2: on the whole, unified governments reduce revenues following surpluses and increase them following deficits. Split-branch governments also adjust but their reaction is, on average, less, and split-legislature governments fail to adjust revenues.

TABLE 1

Simultaneous Estimates of State Revenues and Expenditures, 1968–87

EXPLANATORY VARIABLE AND PARAMETER	NONSOUTHERN						SOUTHERN	
	UNIFIED REPUBLICAN		UNIFIED DEMOCRAT					
	RESTRICTED	UNRESTRICTED	RESTRICTED	UNRESTRICTED	SPLIT BRANCH	SPLIT LEGISLATURE	DEMOCRATS	SPLIT BRANCH
Revenue equation								
Constant, a_1	−57.5	40.3	−139.4	−277.4	−112.5	−134.8	−12.83	−33.7
	(66.6)	(80.1)	172.7)	(118.2)	(40.4)	(58.9)	(31.3)	(59.4)
Republican governor	—	—	—	—	−7.90	5.89	—	—
					(12.2)	(16.4)		
Revenue$_{t-1}$, a_2	.761	.886	.902	.869	.939	.863	.896	.937
	(.063)	(.042)	(.090)	(.070)	(.028)	(.039)	(.026)	(.036)
Income, a_3	.016	.012	.022	.037	.013	.022	.009	.008
	(.007)	(.006)	(.021)	(.010)	(.003)	(.006)	(.003)	(.006)
Federal contribution, a_4	1.016	.196	.220	.523	.329	.417	.242	.246
	(.195)	(.078)	(.161)	(.189)	(.079)	(.094)	(.084)	(.104)
Surplus$_{t-1}$, a_5	−.695	−.419	−.157	−.535	−.128	.067	−.183	−.132
	(.074)	(.089)	(.174)	(.119)	(.058)	(.089)	(.059)	(.108)
Expenditure equation								
Constant, b_1	35.2	63.7	166.7	183.8	56.8	94.8	9.23	−7.76
	(35.5)	(63.8)	(58.6)	(95.1)	(24.1)	(34.2)	(20.9)	(28.2)
Republican governor	—	—	—	—	7.20	−9.00	—	—
					(10.7)	(12.6)		
Revenue$_t$, b_2	.855	.887	.827	.811	.897	.898	.958	.969
	(.031)	(.043)	(.042)	(.049)	(.017)	(.023)	(.018)	(.027)
Unemployment, b_3	9.33	15.57	6.53	6.48	7.20	3.90	1.12	5.70
	(3.15)	(5.49)	(3.33)	(4.66)	(2.19)	(2.67)	(1.49)	(2.48)
Surplus$_{t-1}$, b_4	.023	−.529	−.439	−.418	−.607	−.656	−.546	−.773
	(.031)	(.093)	(.100)	(.100)	(.050)	(.072)	(.054)	(.084)
Number of cases	71	59	65	87	236	113	225	75
System R^2	.94	.95	.97	.89	.96	.96	.94	.96

Sources: *State Government Finances*, published by the Census Bureau, various issues of the *Book of the States*, and the *Statistical Abstract*. Coefficients are unstandardized three-stage least squares regression coefficients, calculated by SST (*Statistical Software Tools*). Standard errors are in parentheses. All monetary variables are measured in thousands of constant 1982–84 dollars per capita. *Restricted* and *unrestricted* refer to constitutional provisions permitting a budget deficit to be carried over from one fiscal period to another. Some state-level dummy variable coefficients are not shown to save space (see n. 22 for details).

On the expenditure side of the model, expenditures closely follow projected revenues, as in our stylization of the budget process. All cases but one (southern Democrats) display a significant cyclical response to the economy as represented by the unemployment rate. We discuss the total effect of the lagged surplus on expenditure (and the spending predictions made by hypotheses 2 and 3) in the next section, since the lagged surplus coefficient (b_4) in this equation does not have a simple interpretation (see Appendix A).

On the whole, the results reported in Table 1 are satisfactory. Our choice to disaggregate by region, partisan grouping, and deficit carryover restriction appears to capture most of the significant state-to-state variation in adjustment processes.[23] Not a single coefficient was significantly estimated with a sign contrary to expectations. The equations fit the data well. The patterns of coefficients we have described support the foundations of the model—targeting to permanent levels of income, response to economic cycles, the stylized budget process, the relevance of deficit carryover laws, and (so far) partisan differences—quite well. The model seems to be free from obvious faults.

Most important, the adjustment parameter estimates (a_5 and b_4) do not pool across partisan configurations, while individual states appear in several different groupings—indeed, sometimes in all possible groupings (outside the South). Thus each "state" as a unit of observation reflects its experience under *different* patterns of partisan control with *different* consequences for fiscal policy. Therefore spending averaged across years by states, for instance, will reflect the varied partisan histories of each state. The disaggregation to state–years as observations, separated into partisan configurations, may be necessary to reveal partisan and institutional effects. Moreover, attempting to designate states as Democratic or Republican may well cause researchers to miss partisan effects, because of the diversity of each state's partisan history.

Reduced-Form Tests: Parties and Composition of Adjustment

Party Differences. Our first hypothesis was that Democrats have a higher—and Republicans a lower—"target" share of state-level income toward which they would push public spending when in office. Since the parties do not typically enjoy unified control of state governments for long periods, we cannot expect observed actual spending differences fully to reflect these underlying differences in goals. We therefore derive partisan differences in spending targets from the reduced-form equation 3 (which, recall, can be calculated from the estimates in Table 1 by the procedure in Appendix A) as shares of *permanent* income, analogous to Alt and Chrystal's (1983) for national governments in Britain and the United States. These permanent income shares are the steady-state levels of expenditure as a share of income *implied* by the dynamic coefficients in Table 1—the levels at which there would be no further systematic drift up or down if a given partisan configuration governed forever. Finding these stable permanent income shares of expenditure that the estimates in Table 1 imply to be indeed higher for Democrats than Republicans would offer further support for hypothesis 1. By transforming the coefficient values from equation 3 for unified nonsouthern Democrats and Republicans,[24] we obtain the needed target spending shares for unified Democratic and Republican governments (and the difference between them and other partisan configurations) (Table 2).

On average, pooling across the coefficients reported in Table 1, columns 3 and 4, the steady state of the model implies that Democrats appear to want state spending to equal 11.06% of permanent personal income (line *a*) while (pooling columns 1 and 2 of Table 1) Republicans want to spend 4.36% (line b). To repeat, this is an underlying, idealized difference revealed by our estimates of how the parties would behave if they had sufficient unified control to have brought fiscal policy to their desired levels and believed they would govern indefinitely with aggregate incomes at sustainable levels, rather than a description of the circumstances in which many parties find themselves much of the time. This implied interparty difference in targets of 6.70 percentage points (line *c*) is marginally statistically significant[25] and amounts to a difference of over $600 per person per year (1982 dollars) in desired spending levels. While this difference may seem too large, much spending is mandated at the state level by other branches and levels of government, limiting the discretion of parties seeking to cut spending. Moreover, while actual spending levels appear higher than these targets, recall that they are also partly financed by federal contributions, so that the actual spending shares financed out of state incomes are probably typically between these two targets.

TABLE 2

Reduced-Form Hypothesis Tests for "Permanent" Expenditures

TYPE OF PARTY CONFIGURATION	PERCENT OF INCOME	STANDARD ERROR
Effects of State Income		
Nonsouthern		
a. Unified Democrats	11.06	(4.0)
b. Unified Republicans	4.36	(1.3)
c. Difference (a − b)	6.70	(4.2)*
d. Split branch	6.65	(2.2)
e. Split legislature	8.78	(3.1)
Southern		
f. Unified Democrats	6.09	(2.4)
g. Split branch	7.92	(1.0)
Effects of federal contributions[a]		
h. Unified Democrats	4.06	(1.5)
i. Unified Republicans	5.18	(1.3)

Note: The permanent effect of an explanatory variable in equation 3 is equal to its coefficient multiplied by the reciprocal of 1 minus the coefficient for lagged expenditures (see n. 24 and Appendix A). For income, this effect is $b_2a_3/(1 - b_2a_2)$. All estimates are based on coefficients reported in Table 1. Averages for unified governments are weighted in proportion to the number of cases in Table 1, cols. 1–4.

[a] Nonsouthern.

*$p < .05$.

In any case, the implication is that a unified Democratic (Republican) government taking over where the actual income share of spending was between the two targets would exert upward (downward) pressure on spending until its target level was reached. Thus our basic, first party-differences hypothesis is statistically sustained for target levels even if not for actual levels of spending. The insignificantly higher levels of actual spending as a share of income in nonsouthern Democratic than in Republican states is partly because the two parties do not hold unified control of government long enough to reach their targets.

Table 2 reveals some other interesting results. First, the target spending share of southern Democrats (line *f*) appears considerably (but not statistically significantly) lower than that of nonsouthern Democrats, consistent with their image as fiscally conservative. Divided cases produce target levels somewhere between those of the parties, but the split-legislature case (line *e*) has a target level closer to that of the unified Democrats, as though the high-demand party usually wins on the spending side in divided legislatures. This was not a feature of our model, though it does not contradict any specific assumption we made. Finally, there is no evidence of party differences in "permanent" dependence on federal aid: both unified partisan groups spend out of federal aid in similar ways, and in ways that seem to imply the expectation of a lot of growth in federal aid (the long-run levels are greater than actual averages),

TABLE 3

Response to Fiscal Imbalance

TYPE OF (NONSOUTHERN) PARTY CONFIGURATION	RESPONSE TO A $1 DEFICIT: REVENUE	SPENDING	TOTAL
Unified Governments	.46 (.06)	.01 (.08)	.45
Republicans			
No Carryover allowed	.69	−.08	.77
Carryover allowed	.42	.11	.31
Democrats			
No Carryover allowed	.16	−.18	.34
Carryover allowed	.54	.14	.40
Split Branch	.13 (.06)	−.12 (.08)	.25
Split Legislature	−.07 (.09)	−.18 (.11)	.11

Note: From equation 3, the revenue response to each dollar of deficit is calculated as $\$(-1)$ times a_5. The spending response is $\$(-1)^*(b_2(a_2 + a_5) + b_4)$. The total response is the revenue response minus the spending response. All estimates are based on coefficients reported in Table 1. Averages for unified governments are weighted in proportion to the number of cases in Table 1, cols. 1–4. Standard errors are in parentheses.

probably because federal aid did in fact grow through much of the period.

Composition of Adjustment. Hypotheses 2, 3, and 5 made predictions about how different partisan configurations would respond to unexpected surpluses and deficits, namely, that unified government would react more in terms of revenues and split governments more in terms of expenditures. We saw that indeed unified governments raise revenues in response to a surprise deficit and split legislatures do not. In contrast, is the total response of spending to a surprise surplus (as before, $b_2(a_2 + a_5) + b_4$) greater in the split case than the unified case? Table 3 contains somewhat mixed answers.

Each row of Table 3 contains the revenue response to an unexpected one-dollar deficit, the expenditure response, and the total response, which is the revenue response minus the spending response. For example, for each dollar of deficit, unified governments raise revenues by 46¢ and spending by 1¢ (for a total response of 45¢) in the next year. The response to a surplus is just the mirror image of this. (Since the permanent planned surplus is assumed zero and adjustments are made every year, we do not calculate "permanent" quantities in this case.) Split-branch governments have on average a much smaller revenue response (13¢) and do also cut spending (by 12¢). Split legislatures, as predicted, have the largest spending response (18¢) but the smallest total response, only 11¢ on the dollar. These qualitative differences between unified and divided governments also appear to exist in the South. All this is broadly consistent with our expectations, but the large standard errors prevent us from regarding it as more than weak confirmation.

Disaggregating the unified governments by party and deficit carryover laws reveals one important qualification. In three out of the four cases, as expected, the revenue response is larger than the spending response, which is insignificantly different from zero. Moreover, the total response is much larger (at least under Republicans) where deficit carryover prohibitions exist, which makes a good deal of sense.[26] However, unified Democrats subject to strict deficit laws do not appear to respond strongly to unexpected deficits with either spending cuts or revenue increases, though we note that both lagged surplus coefficients in this case have large standard errors. Our simulation results below suggest that unified Democrats are more likely to respond to unexpected deficits where strict carryover prohibitions exist. In addition, symmetry with the surplus case implies that these constrained unified Democrats also respond to unexpected surpluses by raising spending. This is consistent with the assumption that they are a high-demand party, but outside our model.[27]

Split-Branch Government. Hypothesis 5 predicted four specific patterns of response by split-branch governments to fiscal shocks, depending on whether the high- or low-demand party controlled the legislature and whether the shock was positive or negative. Few significant results were found, probably because of insufficient data. The mixed quality of these results holds whether or not state-level fixed effects are included in the equation.[28] Of course, since we have not incorporated measurements of V, the partisan identity of the critical veto-override vote, nor state-to-state variations in veto provisions and majorities required for overrides, these anomalies could be due to measurement problems. Unfortunately, we can show that further predictable cases arise under split-branch government depending not only on veto and override provisions, but also on which party last had unified control, so collecting further data is essential (Alt and Lowry 1993). In any case, while the results are promising, we do not carry the analysis of split-branch government further at this time.

Simulation Results

The remaining hypotheses 4, 7, and 8 jointly predict that large cumulative deficits are likelier under split legislatures than unified governments. That is, deficit carryover laws should reduce the propensity of all governments to run deficits but in addition these laws, lacking any enforcement mechanism other than electoral accountability, should be less effective in circumscribing the behavior of split legislatures, other things equal. For reasons already given, we test these hypotheses with simulations. These describe the behavior of "typical" governments representing specific partisan configurations in specific constitutional settings, if each were to remain in power for 10 years in

TABLE 4

Simulation Results

TYPE OF NONSOUTHERN PARTY CONFIGURATION	INITIAL AVERAGE	MEDIAN FINAL	S.E.	90% INTERVAL
Long-run Revenue as % of Income				
Republicans	12.6	12.3	(1.5)	[10.2, 15.0]
Democrats	14.4	15.3	(3.3)	[9.1, 20.0]
Split Legislature	14.1	15.3	(3.4)	[10.0, 20.3]
Long-run Spending as % of Income				
Republicans	12.1	11.5	(1.5)	[9.6, 14.4]
Democrats	13.2	13.0	(2.6)	[8.8, 16.8]
Split Legislature	13.0	13.8	(3.3)	[7.2, 17.4]
Effect of Negative Income Shock on Per Capita Surplus ($)				
Constrained governments				
Republicans	—	−2.1	(4.1)	[−9.3, 1.7]
Democrats	—	−1.7	(27.1)	[−44.5, 45.6]
Split legislature	—	−63.2	(15.4)	[−90.1, −43.1]
Unconstrained governments				
Republicans	—	−17.0	(8.4)	[−31.4, −6.0]
Democrats	—	−49.5	(28.5)	[−97.3, −11.1]
Split legislature	—	−39.1	(28.8)	[−78.7, 14.5]

Note: Simulation methodology is explained in Appendix B. We obtain the 90% interval by omitting the values for the highest and lowest 5% of simulation runs. Descriptive statistics for revenues and spending are averages pooling runs for constrained and unconstrained governments weighted according to the number of cases shown in Table 1. Numbers of cases for constrained and unconstrained split legislatures are 59 and 54, respectively.

average economic circumstances, always allowing for the uncertainty of our estimates. We determine the effects of a surprise recession by comparing two scenarios for each configuration: in one the simulation begins in average economic circumstances, while in the other income is initially reduced by 10%, after which normal growth resumes, and nothing else differs.[29] Given our data, 10 years uninterrupted government by any party configuration is unusually long (and a 10% shock is pretty large). Even so, the simulations give valuable insights into the meaning of the parameter estimates, or the direction in which they indicate the model would go, if governing configurations survived longer than they typically do.

Table 4 gives two main results, of which the first is simply that the models reveal no long-term tendency for any party configuration, be it Democrats, Republicans, or split legislatures, to run deficits per se. The top of the table gives the median simulated long-run outcomes for revenues and expenditures as shares of income, starting in normal, or actual average circumstances. Each line of the table contains the initial value used in the simulation, the median result (and standard error) of one hundred simulated runs, and the interval spanning all but the most extreme five cases at each end. The results show that a 10-year run of unified Republican government slightly reduces revenues as a share of income (by .3% relative to its initial average level), while spending as a share of income declines by a little more. Under unified Democrats, spending is virtually constant, but revenues rise.[30] None of these changes or differences remotely approaches statistical significance. There is a tendency for both revenues and spending to rise under split legislatures, again suggesting the possible importance of bilateral veto power and asymmetries in the desire for tax cuts and spending increases, though again the changes are not large in comparison to the standard errors of the estimates, so that our interpretation is speculative. However, the main point is that such spending increases and decreases as do appear to occur are financed by matching, parallel changes in revenues, regardless of party configuration.

This is important, because when the simulations begin with a major unexpected income shock, the results differ dramatically. The rest of the table shows what happens when the model is simulated after a "surprise recession" (i.e., the model starts with a 10% reduction in income from its actual average value, so that revenues drop and spending is suddenly "too high"). The long-run result under one-party unified government (either party, the results are statistically indistinguishable) subject to a deficit carryover constraint is that the deficit that results from the unexpected recession is entirely *eliminated* through fiscal policy. These long-run outcomes, extra deficits of $1 to $2 per capita attributable to the long-run effects of recessions, are tiny in comparison to the average per capita surpluses of $75 to $100 that existed in these subgroups. By contrast, where no deficit carryover laws exist, the model's estimates imply that both parties allow cumulative deficits larger than in the constrained case. The long-run Democratic deficits (about $50 per capita more than in normal times) are larger than the Republican ones (below $20 per capita), which reflects the relative spending pressures arising from the different targets of the parties. All these differences (except between the parties where no restrictive law exists) achieve significance levels of at least 10%, so they probably should not be ignored.[31] While one could wish for the differences to be sharper, the results are clearly consistent with our initial hypotheses.

The simulation results also indicate that split legislatures would run larger cumulative deficits after recessions but apparently would do so regardless of whether or not the law permits deficits to be carried over from year to year, just as predicted in hypotheses 4 and 8. The differences between the constrained and unconstrained cases for split legislatures are not significant. One can infer that overall the average extra long-term deficit after a recession under a split legislature would be on the order of $50 per capita regardless of the legal environment. The difference between split legislatures and unified governments subject to deficit carryover laws *is* statistically significant[32] and substantively significant as well. If these deficit carryover laws do actually work but only for unified governments, it seems likely that the ability to blame one party for undesirable outcomes is an important element in their effectiveness. In any event, these simulations confirm in the long run what Table 3 hinted at in the short run: unified governments subject to constraints respond more (and more quickly) to income shocks than do unconstrained unified governments, and more than split legislature governments regardless of whether the latter have legal restraints.

CONCLUSION

Our central message is straightforward. At the state level, fiscal outcomes—and fiscal instability in particular—result from an interaction of economic and political circumstances. Divided government matters, institutions matter, and party control matters. Our estimates suggest that even before the widespread problems of the last few years, recessions trigger state-level fiscal problems but that combinations of party and institutional control on the one hand and legal restrictions on fiscal policy on the other help explain their persistence. We show how, faced with an unexpected recession or inheriting a deficit, unified party governments not subject to deficit carryover laws might allow it to grow (if they remained in office) while those subject to such laws eliminate deficits more quickly. States with split legislatures also adjust less, regardless of the legal situation, in large part because divided legislatures do not appear to adjust revenues in response to surpluses and deficits.

Other recent research consistent with our conclusions came to our attention after we had completed most of the analysis. Using a wholly independent source of data covering fiscal 1988–92 (the period immediately following ours but containing widespread deficits), Poterba (1994) shows that (1) unified state governments adjust more quickly to unexpected deficit shocks than do split governments, (2) unified governments make most of their adjustment with tax changes (rather than spending) and do so to a greater extent than split governments, and (3) where rules affecting budget balance are stronger, governments adjust more quickly to offset unexpected deficit shocks. He restricts his analysis to within-year changes, his definitions of the dependent and independent variables are slightly different from ours, and he makes no attempt to characterize the differences between parties; but his results are gratifyingly consistent with ours. They suggest that were our model and data used to forecast five further years (when the data are available), it should have some success.

Our results add a twist to recent interpretations of federal deficits in the 1980s. While we offer further evidence of the role of a divided legislature in producing deficits (relevant to the federal experience in the first half of the 1980s), we also show that unified Democratic government not subject to deficit carryover constraints (the final fiscal years under Carter as well as the present situation at the federal level) should not be expected to reduce deficits in the face of recessionary pressures. Somewhat pessimistically, a literal translation of Tables 3 and 4 to the federal level suggests that unified Democratic government (or even to some extent split-branch government with a Democratic Senate) in the early 1980s would have raised taxes further, initially achieving a little more deficit reduction but (as Table 4 suggests) probably not achieving long-run results very different from those of a split legislature. Naturally, such interpretations are speculative, especially since our characterization of the state-level parties as representing high- and low-demand constituencies may not apply well to the federal level.

On the other hand, the findings about the effect of deficit carryover laws are relevant to the national debate over balanced budget amendments. From our results, we conclude that laws affecting the end of the fiscal year are most likely to be effective and that they probably are effective (if sufficiently transparent in their requirements) by forcing explicit recognition of the past year's shortfall in the next year's plans. Moreover, the fact that they only affect the conduct of unified governments suggests that, ultimately, electoral accountability (or the threat of it) is the mechanism of enforcement, probably through the laws' enhancing the ability of media and organized groups to mobilize public opinion. A study of the effects of fiscal outcomes on election outcomes would be informative—as distinct from the state economy, for whose cyclical movements state politicians probably cannot reasonably be blamed (Chubb 1988; Peltzman 1987). Finally, we note that the desirability of such balanced budget laws hinges on how their benefits (fiscal responsibility) stack up against their costs (inflexibility in economic stabilization, much more significant at the federal than the state level). Taking our results with Poterba's suggests that there is a gain in responsibility. Husted, Morton, and Waller (1992) find that even under these laws the loss of fiscal discretion is far from complete, so there is plenty of room left for debate.

Two methodological comments follow from the result that at the state level the Democrats have a significantly higher, and the Republicans a lower, target share of personal income for state spending. First, our model integrates the roles of economic cycles, income, federal contributions, and party control in determining state spending, rather than treating them as rival hypotheses to be tested individually. The fact that all these factors simultaneously have significant roles suggests that significant omitted-variables problems bias bivariate hypothesis tests. Moreover (as we have noted), almost all the states move in and out of different partisan configurations over time, and these different partisan configurations produce different fiscal outcomes. Thus efforts to attach "average" or "typical" partisan tags to individual states over time may swamp partisan differences with measurement error.

Finally, we believe the analysis of split-legislature outcomes contributes to the ongoing theoretical debate over the consequences of divided government between those who believe it is a moderating influence on policy (e.g., Alesina and Rosenthal 1994) and those who believe that coordination problems in divided government can impose significant costs (e.g., Roubini and Sachs 1989). We find little evidence for moderation, especially if spending targets are actually higher in split legislatures. However, any extra spending is normally financed through taxation, and there is no tendency for divided government automatically to produce gridlock. Divided government may produce fiscal instability but only conditional on experiencing economic instability. Naturally, no one case will settle the debate, but more concrete results can only be beneficial.

APPENDIX A: DERIVATION OF REDUCED-FORM EXPENDITURE FUNCTION

We start with the following structural model:

$$R_t = a_1 + a_2{}^*R_{t-1} + a_3{}^*Y_t + a_4{}^*F_t + a_5{}^*S_{t-1} + v_t \qquad (1)$$

$$E_t = b_1 + b_2{}^*R_t + b_3{}^*U_t + b_4{}^*S_{t-1} + w_t \qquad (2)$$

After dropping all time subscripts except for lagged values, we can substitute for R in equation 2 to get

$$E = b_1 + b_2{}^*(a_1 + a_2{}^*R_{t-1} + a_3{}^*Y + a_4{}^*F + a_5{}^*S_{t-1}) + b_3{}^*U + b_4{}^*S_{t-1} + b_2{}^*v + w.$$

Rearranging terms and taking expected values gives

$$E = (b_1 + b_2a_1) + b_2a_3{}^*Y + b_2a_4{}^*F + b_3{}^*U + b_2a_2{}^*R_{t-1} + (b_2a_5 + b_4){}^*S_{t-1}.$$

Note that $(b_2a_2 - b_2a_2)^*E_{t-1} = 0$. Add $(b_2a_2 - b_2a_2)^*E_{t-1}$ to the righthand side to get

$$E = (b_1 + b_2a_1) + b_2a_3{}^*Y + b_2a_4{}^*F + b_3{}^*U + b_2a_2{}^*E_{t-1} + b_2a_2{}^*(R_{t-1} - E_{t-1}) + (b_2a_5 + b_4){}^*S_{t-1}.$$

Rearranging and collecting terms gives the reduced form:

$$= (b_1 + b_2a_1) + b_2a_2{}^*E_{t-1} + b_2a_3{}^*Y + b_2a_4{}^*F + b_3{}^*U + (b_2{}^*(a_2 + a_5) + b_4){}^*S_{t-1}. \qquad \textbf{(3)}$$

APPENDIX B: MONTE CARLO SIMULATION METHOD

Our Monte Carlo approach was to do repeated but stochastically different runs by states with and without budget carryover constraints for each nonsouthern partisan group (omitting split-branch), and in each case with and without an adverse initial outcome shock. A set of simulations in each of these 12 cases (three partisan grouping times two legal situations times two income situations) requires carrying out steps 1 through 6.

1. For each configuration of party control and budget constraint, our 3SLS produces a nine-by-nine variance–covariance matrix for the structural model coefficient estimates in the appropriate column of Table 1, and a two-by-two variance–covariance matrix for the additive residual terms.[33] Using those matrices, for each configuration repeat steps 2–6 one hundred times.
2. Construct the parameter values for the structural model by adding a randomly drawn stochastic term (with variance equal to that estimated) to the coefficient point estimates.
3. Set the initial values of the variables for the base case equal to the means for the appropriate partisan group data subsample. For the negative shock case, use the same initial values except for income, which is set 10% lower than the mean.
4. Calculate the revenue and expenditures for time 1, including randomly drawn additive residual terms (from normal populations with standard deviations given by the regression standard errors) in each equation. The same residuals are applied to both base case and shock case.
5. Calculate the surplus for both cases at time 1 as revenues minus expenditures.
6. Continue updating through time 10, drawing a different pair of additive residuals for each iteration. Income is updated each period according to the grand mean annual growth rate (1.35%). The structural parameter values remain the same for all 10 iterations.

The result is a matrix of expenditures, revenues, and surpluses for one hundred runs of 10 iterations each, with randomly different parameter values for each run and randomly different additive residuals for each time period of each run. The same stochastic terms and growth rate are applied throughout to the base case and shock case, so that the difference between the cases for a given configuration of party control and budget constraints is attributable solely to the fact that the shock case begins with a one-time, 10% drop in income.

Notes

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1. For political science studies of parties and policy, see Chubb 1988; Erikson, Wright, and McIver 1989; Garand 1988. For studies of budgeting, executives, and legislatures, see Behn 1991; Beyle 1992; Clynch and Lauth 1991. For a detailed study of powers, techniques, and institutions, see Hutchinson and James 1988. Public finance stresses the effects of fiscal health, the contributions of recession and recessionary surprises, changes in fiscal federalism and federal aid, differential cost elements, mandates at the state and national level, ability to pay, and indicators of need or demand (see Gramlich 1991). Some public finance studies of fiscal institutions are cited in Poterba 1994, especially Carter and Schap 1990 and Dearden and Husted 1990.

2. The 10 states include California, Connecticut, Illinois, Louisiana, Massachusetts, Maine, New York, North Carolina, Ohio, and Pennsylvania. In 9 cases the same party did not control the executive and both houses of the legislature. In the tenth, Louisiana, there was a major factional split between governor and legislature within the Democratic party.

3. In some models parties differ in the composition rather than level of spending (McCubbins 1991; Tabellini and Alesina 1990). For the conditions under which there is full policy convergence, see Alesina and Rosenthal 1994.

4. A full formal analytical model of the policy choices we analyze would also endogenize voters' choices and the resulting complex effects of transitions between types of partisan control. Alt and Lowry (1993) offer preliminary analyses of the latter.

5. No precise definition of state-by-state reversion levels exists. When a budget has not been passed by the beginning of the fiscal year, some states pass short-term continuing resolutions, based on prior-year spending levels. Experts believe that courts would not allow state government to shut down mandated services for long, so some continuing operation is always likely. A recent effort to codify the reversion level at the status quo ante by voter initiative in California was defeated.

6. A consequence of our use of a static model to analyze deficits is that we cannot deal directly with intertemporal aspects of debt, such as the desire to use debt to precommit political adversaries (Tabellini and Alesina 1990), nor do we construct a dynamic model of the deficit like that of Roubini and Sachs (1989). However, our steady-state focus enables us to examine partisan spending targets empirically and yields other results described. Two models with intertemporal and partisan differences in incentives to run deficits predict that borrowing should increase under unified governments in polarized settings (especially when they are uncertain of reelection) but offer no empirical tests of the predictions (Cohen and Noll 1984; Tabellini and Alesina 1990). Husted, Morton, and Waller (1992) failed to find significant preelection political cycles in fiscal policy at the state level. Alt and Stewart (n.d.) examine the possibility of reconciling these approaches.

7. We believe that unforeseen revenue shocks (i.e., governments' greater control over the actual level of expenditures than of revenues) are more likely at the state level, where the government does not assume responsibility for defense or aggregate demand management. We lack data on targeted levels of spending and revenue for all states and years but note that recent research has found few systematic political and institutional effects on the accuracy of revenue forecasting (Bretschneider and Gorr 1992).

8. Note that we do not assume that one party is per se more desirous of borrowing than the other and thus predict no direct partisan effects on the level of the deficit. It could be the case that parties differ in both their desired level and composition of spending (McCubbins 1991) but we have not formally pursued this added complication.

9. Even a unified party government need not have a single bliss point, but again we present the simplest case. The closer the bliss points for the executive and legislature, the less likely there will be any adjustment in the expected level of spending, as we believe our discussion of split-branch governments demonstrates.

10. We assume that government can believe a revenue shock is transitory or, if not, that permanent income is producing more or less revenue than before.

11. Again, we simplify away institutional details of the bargaining game (e.g., conference committees) that would affect the identity of critical legislators. The key point is that these legislators have bilateral veto power over each other (McCubbins 1991).

12. Alesina and Drazen (1991) show (for heterogeneous interest groups bargaining over the tax burdens imposed by fiscal stabilization) that there is a subgame perfect Nash equilibrium where the time until the first concession is positive. Indeed, if the cost of delay is sufficiently low, no group may concede.

13. Another complication we ignore is that if one party nominally has the power to pass a bill in both houses but lacks the power to override a veto in one chamber, then there are two veto locations. We are currently collecting the data on override requirements and partisan majorities that will allow this possibility and others derived from the identity of V to be tested directly.

14. Where the executive has a line-item veto, the coalition situation is more complicated. There is likely to be considerable behind-the-scenes negotiations to determine exactly where the critical veto-override point is and what the indifference curves look like (see Beyle 1983; Carter and Schap 1990) and significant delay becomes more possible.

15. The data come from the Census Bureau's *State Government Finances*, various issues of the *Book of the States*, and the *Statistical Abstract*.

16. In our sample, states with unified Republican government have lower unemployment rates than Democratic states, partly because unified Republican government and lower unemployment were more common early in the period.

17. We group the 11 states of the former Confederacy (Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, the Carolinas, Tennessee, Texas, Virginia) with the four border states in which Republicans failed to control even one chamber of the legislature for any year from 1968 to 1987 (Kentucky, Maryland, Oklahoma, West Virginia) as "southern." We omit Nebraska because it has a nonpartisan, unicameral legislature, Minnesota from 1968 to 1972 because the legislature was nonpartisan, and Maine from 1975 to 1978 because it had an independent governor, giving a total dataset of 931 state–years (631 outside the South). Control of legislative chamber is determined by which party held more seats. Where both parties held an equal number of seats, the chamber is assigned the party designation of the executive.

18. Two southern states had only unified Democratic government, while the other 13 also had split-branch government.

19. The 26 states include Alabama, Arizona, Arkansas, Colorado, Florida, Georgia, Idaho, Indiana, Iowa, Kansas,

Maine, Michigan, Mississippi, Missouri, New Jersey, New Mexico, North Carolina, Ohio, Oklahoma, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Utah, and West Virginia.

20. The National Association of State Budget Officers (NASBO) publishes a list of whether the laws have "enforcement provisions"—derived from a survey of state budget officers—but had no further information about exactly what this variable means. The ACIR has also published a 10-point scale of budget balance provision severity (see Poterba 1994), and the NASBO has released a classification of deficit carryover prohibitions that would remove Arizona, Colorado, Michigan, and Pennsylvania from our list of 26 (see *State Tax Notes*, 27 July 1992). We have rerun our main results using this alternate listing: where the number of cases gets smaller, standard errors become larger, sometimes appreciably. The qualitative differences in behavior we describe still shine through, however.

21. The first three terms of equation 1 are an adaptive expectations setup in which expected (permanent) revenues are implicitly a function of past values of state income and federal contributions. This, adjusted for last year's unexpected surplus/deficit, yields the revenue forecast that starts the annual budget cycles, exactly as in our stylized description. Our formulation, with the revenue forecast made first (to finance targeted total spending), reflects our view of the "typical" process in the states, based on a mix of case analyses and discussions with budget officers. Naturally, which party set last year's fiscal policy affects the size of adjustment in transition periods. Moreover, in this model transitory demand increases expected borrowing, because it increases expected spending but not expected revenues, consistent with the distinction made in Barro's work. Empirically, these two equations contain the same explanatory variables used by Barro (1986) and Roubini and Sachs (1989). Because of incomplete data, we omit interest rates and expected prices, so we cannot judge the optimality of the borrowing that results. Finally, note that we do not measure fiscal surplus or deficit by examining additions to public debt (as an exact replication of Barro's model would require) but instead focus on the fiscal year deficit or surplus measured by the difference between total revenues and expenditures. We do this because states tend to use debt to finance long-run capital projects such as highway construction, so that there is no necessary connection between the yearly balance of spending and revenues and the total debt outstanding. In fact, the level of outstanding debt grew quite rapidly during the 1980s for some states that showed repeated, large surpluses on their annual balance sheets.

22. We prefer to report split-sample estimates (equivalent to a fully interactive, variable-intercepts, variable-slopes model) to keep the interpretation as clear as possible, even if it means accepting a little inefficiency from estimating some extra coefficients for interactions that are truly irrelevant. We could certainly economize by a few parameters: for instance, the two Unified Democrat spending equations are almost identical, and we do not claim that the partisan and regional types we identify differ in every way. However, the inclusion of a few irrelevant parameters should introduce no bias and will not lead to exaggerated claims of significant differences between types. To deal with fixed effects while avoiding excessive tracking of small numbers of outlying observations, variables were only included in any equation for states with nine or more observations in that subsample. In our data, this guarantees that a state appears as a separate variable in at most one partisan subcase, and no state variables appear that are not statistically significant in at least one equation. The states meeting these criteria include Connecticut, Iowa, Indiana, Massachusetts, Missouri, New Hampshire, New Jersey, New Mexico, Montana, Rhode Island, South Dakota, and Vermont.

23. The model in Tables 1 and 2 survived various hypothesis tests (conventional F-tests for the joint contribution of several coefficients based on differences in sums of squared errors). It offers significant improvement over a simple model pooling all observations and estimating the nine parameters of equations 1 and 2, as well as models that do not control for deficit carryover provisions and unified governments. Moreover, it strictly dominates (fewer parameters, better fit) a model in which each state has a separate intercept in each equation. Even more important, a model in which each state has a separate *slope* and *intercept* does *not* offer significant improvement over that in Tables 1 and 2. Finally, incorporation of state-level fixed effects and separation by deficit carryover laws in the split-branch and split-legislature cases failed to produce significant improvement. All these tests are conservative in that we have not engaged in extensive specification searching to minimize the number of included parameters.

24. Equation 3 has a Koyck lag structure, which is mathematically equivalent to an equation that relates current expenditures to the weighted average (with geometrically declining weights) of all current and past values of income, federal contributions, unemployment, and surpluses or deficits. If we set each of the past and present values of the explanatory variables equal to their "permanent" expected value, then the effect on spending becomes the coefficient in equation 3 multiplied by the reciprocal of 1 minus the coefficient for lagged expenditures, $b_2a_3/(1 - b_2a_2)$. Figures for all unified governments are averages calculated from Table 1, weighted in proportion to the number of cases in columns 1–4.

25. Note that the expression for the permanent income contribution to spending is a nonlinear function of several parameters. We therefore obtained an estimate of the standard deviation by simulation methods, using the estimated coefficients' variance–covariance matrix as a guide.

26. Remember that these laws often specify the conditions under which deficits must be extinguished rather than forbidding them outright, so the effectiveness of the laws is not incompatible with the existence of at least transitory borrowing. We experimented with regressions using other definitions of the laws (governor must submit or legislature must pass a balanced budget) and found no significant differences according to whether or not such laws existed.

27. Similarly, to some extent nonsouthern split legislatures spend an unexpected surplus rather than cut taxes. We had assumed for modeling purposes that legislators were equally averse to sustained surpluses and deficits and that adjustment in either direction would thus be equally difficult. In a period of persistent surpluses, a significant positive spending coefficient means that split legislatures are unable to exercise discipline in holding the line on spending following an unexpectedly large surplus, spending "free money" under bilateral veto pressure. That is, the high-demand party wants the extra spending more than the low-demand party wants the tax cut.

28. For Democratic legislatures facing Republican governors, the pattern of results confirms the hypothesis. If the shock is positive, spending rises, though only by a little. If the shock is negative, revenues subsequently rise (significantly) and spending falls. When Republican legislatures combine with Democratic governors, an unexpected surplus does result in lowered revenues. However, in this case the expected spending decrease does not appear, nor does spending decrease following an unexpected deficit in this case. However, we note that again (as in the split-legislature case), the anomalies arise over the spending behavior of Republican legislatures, so it could be that the model is too limited as well. Also, there are fewer cases of actual deficits in the present data, so we took the expedient course of letting the surplus/deficit split fall at the average shock in each case. Even so, there are only slightly over a hundred cases of each partisan type, so this means estimating nine parameters from 50 cases, leaving little room for adding state-level fixed effects or the effects of deficit carryover laws.

29. Intuitively, one simulation can be described as follows. Use the coefficients from a column of Tables 1 or 2—with the independent variables set to mean values for that column's subgroup of cases—to solve for revenues, then expenditures, and thus a new surplus. With this new surplus, let income

grow at an average rate, hold everything else constant, add a random shock, and solve again, iteratively, until 10 periods are complete. Do one hundred such interated calculations, each of which differs from the others only in its random elements which represent pure shocks in each period plus the uncertainty surrounding the parameter estimates. To isolate the effects of an initial recession, rerun an identical (random elements apart) set of simulations save only that in the first period of each income is reduced by 10% and see what effect this change of starting conditions has in the long run. The exact construction is spelled out in Appendix B. The coefficients were based on regressions with data to which population adjustments have been made which differ slightly from those reported in Table 1.

30. The differences between parties, particularly in long-run revenue levels, are clearer and approach statistical significance at conventional levels in the cases where strict deficit laws exist. We are unsure how to interpret this.

31. Based on Table 4, the results are that for the difference between Republicans and Democrats, conditional on a carryover being allowed, $t = 1.25$ ($p < .15$) and that for the difference between states allowing carryovers and no carryovers, conditional on unified Republicans, $t = 1.58$ ($p < .10$) and conditional on unified Democrats, $t = 1.35$ ($p < .10$).

32. The results are that for the difference between Republicans and split legislatures, conditional on no carryover, $t = 3.87$ ($p < .005$) and for the difference between Democrats and split legislatures, conditional on no carryover, $t = 2.19$ ($p < .025$). The numbers of cases for constrained and unconstrained split legislatures are 59 and 54, respectively.

33. The coefficients for split-legislature cases separated by carryover restrictions did not differ significantly from each other; thus the pooled estimates were reported in Table 1.

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DOES ATTACK ADVERTISING DEMOBILIZE THE ELECTORATE?

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We address the effects of negative campaign advertising on turnout. Using a unique experimental design in which advertising tone is manipulated within the identical audiovisual context, we find that exposure to negative advertisements dropped intentions to vote by 5%. We then replicate this result through an aggregate-level analysis of turnout and campaign tone in the 1992 Senate elections. Finally, we show that the demobilizing effects of negative campaigns are accompanied by a weakened sense of political efficacy. Voters who watch negative advertisements become more cynical about the responsiveness of public officials and the electoral process.

It is generally taken for granted that political campaigns boost citizens' involvement—their interest in the election, awareness of and information about current issues, and sense that individual opinions matter. Since Lazarsfeld's pioneering work (Berelson, Lazarsfeld, and McPhee 1954; Lazarsfeld, Berelson, and Gaudet 1948), it has been thought that campaign activity in connection with recurring elections enables parties and candidates to mobilize their likely constituents and "recharge" their partisan sentiments. Voter turnout is thus considered to increase directly with "the level of political stimulation to which the electorate is subjected" (Campbell et al. 1966, 42; Patterson and Caldeira 1983).

The argument that campaigns are inherently "stimulating" experiences can be questioned on a variety of grounds. American campaigns have changed dramatically since the 1940s and 1950s (see Ansolabehere et al. 1993). It is generally accepted that television has undermined the traditional importance of party organizations, because it permits "direct" communication between candidates and the voters (see Bartels 1988; Polsby 1983; Wattenberg 1984, 1991). All forms of broadcasting, from network newscasts to talk show programs, have become potent tools in the hands of campaign operatives, consultants, and fund-raisers. In particular, paid political advertisements have become an essential form of campaign communication. In 1990, for example, candidates spent more on televised advertising than any other form of campaign communication (Ansolabehere and Gerber 1993).

We are now beginning to realize that the advent of television has also radically changed the nature and tone of campaign discourse. Today more than ever, the entire electoral process rewards candidates whose skills are rhetorical, rather than substantive (Jamieson 1992) and whose private lives and electoral viability, rather than party ties, policy positions, and governmental experience, can withstand media scrutiny (see Brady and Johnston 1987; Lichter, Amundson, and Noyes 1988; Sabato 1991). Campaigns have also turned increasingly hostile and ugly. More often than not, candidates criticize, discredit, or belittle their opponents rather than promoting their own ideas and programs. In the 1988 and 1990 campaigns, a survey of campaign advertising carried out by the *National Journal* found that attack advertisements had become the norm rather than the exception (Hagstrom and Guskind 1988, 1992).

Given the considerable changes in electoral strategy and the emergence of negative advertising as a staple of contemporary campaigns, it is certainly time to question whether campaigns are bound to stimulate citizen involvement in the electoral process. To be sure, there has been no shortage of hand wringing and outrage over the depths to which candidates have sunk, the viciousness and stridency of their rhetoric, and the lack of any systematic accountability for the accuracy of the claims made by the candidates (see Bode 1992; Dionne 1991; Rosen and Taylor 1992). However, as noted by a recent Congressional Research Service survey, there is little evidence concerning the effects of attack advertising on voters and the electoral process (see Neale 1991).

A handful of studies have considered the relationship between campaign advertising and political participation, with inconsistent results. Garramone and her colleagues (1990) found that exposure to negative advertisements did not depress measures of political participation. This study, however, utilized student participants and the candidates featured in the advertisements were fictitious. In addition, participants watched the advertisements in a classroom setting. In contrast to this study, an experiment reported by Basil, Schooler, and Reeves (1991) found that negative advertisements reduced positive attitudes toward both candidates in the race, thereby indirectly reducing political involvement. This study, however, was not conducted during an ongoing campaign and utilized a tiny sample, and the participants could not vote for the target candidates. Finally, Thorson, Christ, and Caywood (1991) reported no differences in voting intention between college students exposed to positive and negative advertisements.

We assert that campaigns can be either mobilizing or demobilizing events, *depending upon the nature of the messages they generate*. Using an experimental design that manipulates advertising tone while holding all other features of the advertisements constant, we demonstrate that exposure to attack advertising in and of itself significantly decreases voter engagement and participation. We then reproduce this result by demonstrating that turnout in the 1992 Senate campaigns was significantly reduced in states where the tone of the campaign was relatively negative. Finally, we address three possible explanations for the demobilizing effects of negative campaigns.

EXPERIMENTAL DESIGN

There is a vast literature, both correlational and experimental, concerning the effects of televised advertisements (though not specifically negative advertisements) on public opinion (for a detailed review, see Kosterman 1991). This literature, however, is plagued by significant methodological shortcomings. The limitations of the opinion survey as a basis for identifying the effects of mass communications have been well documented (see Bartels 1993; Hovland 1959). Most importantly, surveys cannot reliably assess exposure to campaign advertising. Nor is most of the existing experimental work fully valid. The typical experimental study, by relying on fictitious candidates as the "target" stimuli, becomes divorced from the real world of campaigns. Previous experimental studies thus shed little evidence on the interplay between voters' existing information and preferences and their reception of campaign advertisements. When experimental work has focused on real candidates and their advertisements, it is difficult to capture the effects of particular characteristics of advertising because the manipulation confounds several such characteristics (Ansolabehere and Iyengar 1991; Garramone 1985; Pfau and Kenski 1989). That is, a Clinton spot and Bush spot differ in any number of features (the accompanying visuals, background sound, the voice of the announcer, etc.) in addition to the content of the message. Thus there are many possible explanations for differences in voters' reactions to these spots.

To overcome the limitations of previous research, we developed a rigorous but realistic experimental design for assessing the effects of advertising tone or valence[1] on public opinion and voting. Our studies all took place during ongoing political campaigns (the 1990 California gubernatorial race, the 1992 California Senate races, and the 1993 Los Angeles mayoral race) and featured "real" candidates who were in fact advertising heavily on television and "real" voters (rather than college sophomores) who on election day would have to choose between the candidates whose advertisements they watched. Our experimental manipulations were professionally produced and could not (unless the viewer were a political consultant) be distinguished from the flurry of advertisements confronting the typical voter. In addition, our manipulation was unobtrusive; we embedded the experimental advertisement into a 15-minute local newscast.

The most distinctive feature of our design is its ability to capture the casual effects of a particular feature of campaign advertisement—in this case, advertising tone or valence. The advertisements that we produced were identical in all respects but tone and the candidate sponsoring the advertisement. In the 1992 California Senate primaries, for example, viewers watched a 30-second advertisement that either promoted or attacked on the general trait of "integrity." The visuals featured a panoramic view of the Capitol Building, the camera then zooming in to a closeup of an unoccupied desk inside a Senate office. In the "positive" treatments (using the example of candidate Dianne Feinstein), the text read by the announcer was as follows:

> For over 200 years the United States Senate has shaped the future of America and the world. Today, California needs honesty, compassion, and a voice for all the people in the U.S. Senate. As mayor of San Francisco, Dianne Feinstein *proposed* new government ethics rules. She *rejected* large campaign contributions from special interests. And Dianne Feinstein *supported* tougher penalties on savings-and-loan crooks.
>
> California *needs* Dianne Feinstein in the U.S. Senate.

In the "negative" version of this Feinstein spot, the text was modified as follows:

> For over 200 years the United States Senate has shaped the future of America and the world. Today, California needs honesty, compassion, and a voice for all the people in the U.S. Senate. As state controller, Gray Davis *opposed* new government ethics rules. He *accepted* large campaign contributions from special interests. And Gray Davis *opposed* tougher penalties on savings-and-loan crooks.
>
> California *can't afford a politician* like Gray Davis in the U.S. Senate.

By holding the visual elements constant and by using the same announcer, we were able to limit differences between the conditions to differences in tone.[2] With appropriate modifications to the wording, the identical pair of advertisements was also shown on behalf of Feinstein's primary opponent, Controller Gray Davis, and for the various candidates contesting the other Senate primaries.

In short, our experimental manipulation enabled us to establish a much tighter degree of control over the tone of campaign advertising than had been possible in previous research. Since the advertisements watched by viewers were identical in all other respects and because we randomly assigned participants to experimental conditions, any differences between conditions may be attributed only to the tone of the political advertisement (see Rubin 1974).

The Campaign Context

Our experiments spanned a variety of campaigns, including the 1990 California gubernatorial election, both of the state's 1992 U.S. Senate races, and the 1993 mayoral election in Los Angeles. In the case of the senatorial campaigns, we examined three of the four primaries and both general election campaigns.

The campaigns we examined were all characterized by extensive broadcast advertising and, in most cases, by frequent use of negative or attack advertising.

We used the same design for all of the campaigns under investigation. That is, we manipulated advertising valence within the identical audiovisual framework. The content of the experimental advertisement, however, varied across campaigns. In general, the experimental advertisements focused on issues or themes that were particularly salient in the various campaigns. In the 1990 gubernatorial race, we created advertisements that dealt with the issues of crime and pollution. In the positive conditions, the sponsoring candidate was presented as "tough" on crime and a protector of the environment. In the negative versions, the opponent was depicted as "soft" on crime and indifferent to the quality of the environment. (Samples of the text and accompanying visuals of the experimental manipulations are provided in Appendix A.)

The experimental advertisements for the 1992 Senate primaries dealt with either the candidates' personal integrity or competence. In the case of integrity (discussed in the given examples), the advertisement described the candidate as either honest and a supporter of campaign reform or as dishonest and an opponent of reform. In the case of competence, the advertisement asked voters to consider the sponsor's "ability, determination, and leadership" (or the absence of these characteristics in the opponent).

During the Senate general election campaign, we shifted the focus of the advertisements to the issue of unemployment. The condition of the state's economy and the significant loss of jobs (unemployment had reached 10% in September) were the overriding issues in both races. All four candidates aired advertisements promising to reverse the state's economic decline. Our treatment advertisements depicted the sponsor or opponent as an advocate or critic of government-subsidized job training and industrial modernization programs.

Finally, one of our studies concerned the nonpartisan election for mayor of Los Angeles between Richard Riordan and Michael Woo. Here, the manipulation dealt with the candidates' integrity and discussed the degree to which the candidates' campaign promises to increase job opportunities and reform city government were consistent with their past actions.

In summary, our experimental advertisements dealt with a variety of campaigns and themes. In all cases, however, the advertisements corresponded to the actual focus of campaigns. In their content, the experimental advertisements closely reflected the advertisements aired by the candidates.

Subjects and Procedure

We recruited subjects by multiple methods including advertisements placed in local newspapers, flyers distributed in shopping malls and other public venues, announcements in employer newsletters, and by calling names from voter registration lists. Subjects were promised payment of $15 for participation in an hour-long study of "selective perception" of local news programs.

Although the "sample" was obviously nonrandom, our participants resembled the composition of the greater Los Angeles area. Across all the experiments, 56% of the participants were male, 53% were white, 26% were black, 12% were Hispanic, and 10% were Asian. The median age was 34. Forty-nine percent of the participants claimed affiliation with the Democratic party, 24% were Republicans, and 21% were independents. Forty-four percent were college graduates, with the balance being evenly divided between high school graduates and individuals with some college.[3]

The experiments were conducted at two separate locations: West Los Angeles and Costa Mesa (Orange County). The former is a heavily Democratic area, while the latter, an affluent suburb of Los Angeles, is predominantly Republican. The experimental facilities in both locations were identical—a three-room office suite consisting of two viewing rooms and a separate room for completion of questionnaires (in addition to a reception area). The viewing rooms were furnished casually with sofas and easy chairs. Participants could browse through newspapers and magazines and snack on cookies and coffee.

When participants telephoned the facility they were scheduled for a particular time period of their choice. Experimental sessions were available from 10:00 A.M. to 8:00 P.M., Monday through Saturday. The typical session consisted of two to three participants.

On arrival, subjects were given an instruction sheet informing them that the study concerned selective perception of local newscasts. They then completed a short pretest questionnaire concerning their social background, media activities, and political interest. Following completion of the pretest, participants were taken to a viewing room, where, depending upon the condition to which they had been assigned,[4] they watched a 15-minute (complete with commercials) videotape recording of a recent local newscast (described to participants as having been selected at random).

The experimental or "treatment" advertisement was inserted into the first commercial break midway through the tape. The political spot was shown always in the middle position in a three-advertisement break. As described, the advertisements in the various conditions were identical in all respects except for the factors of valence and source.

Following completion of the videotape, participants completed a lengthy posttest questionnaire tapping their beliefs and opinions on a wide range of campaign issues. Of course, we also ascertained participants' voting intentions and general level of involvement in the campaign. On completion of the posttest, participants were debriefed and paid.

ANALYSIS AND RESULTS

We shall limit our analyses to the effects of negative advertising on intention to vote. In our post-test questionnaire, we ascertained whether participants were registered to vote. Using registration as a filter, we then asked, "Looking forward to the November election, do you intend to vote?" (In the case of the primary election study, the question was worded accordingly.) We identified "likely voters" as those who were both registered and who stated their intention of voting.[5]

In analyzing our experimental data, we pooled the gubernatorial study, the various senatorial studies, and the mayoral study into a single data set. While the effects of attack advertising are tempered by campaign-specific constraints, including the background of the candidates and the specific content of their advertising, we are especially interested in the average effect, if any, of advertising valence.[6] Moreover, pooling the separate studies makes it possible to obtain reliable estimates of the demobilizing effects of attack advertising.

After pooling, we compared the percentage of viewers classified as likely voters among participants who watched the positive and negative versions of the experimental advertisements. The demobilization hypothesis predicts that exposure to negative advertising will lower the percentage of likely voters. Among those who watched a positive advertisement, 64% intended to vote. Among participants who saw a product advertisement instead of a political one, 61% intended to vote. Among participants who were exposed to the negative versions of the campaign advertisement, only 58% were likely to vote. A one-way analysis of variance yielded an F-statistic of 2.2, significant at the .11 level.[7]

The decision to vote depends upon aspects of the campaign other than advertising valence. In addition, some people are more likely to vote than others, regardless of the nature of the campaign. To capture these contextual and dispositional effects on turnout, we regressed intention to vote (using a logistic regression) on advertising tone and a set of dummy variables corresponding to specific elections, as well as various indicators of individual differences. Because the positive and negative advertisements exerted symmetric effects on voting intention, we specified advertising tone as a trichotomy corresponding to positive advertisement (+1), no political advertisement (0), and negative advertisement (−1). The individual difference variables included the frequency with which people said they followed public affairs, prior voting history, the "match" between viewers' and the candidates' gender and party identification, age, race, and education. This multivariate analysis, in essence, estimates the independent effects of the campaign advertising stimulus on voting intention above and beyond campaign-specific influences and personal predispositions.

Table 1 presents the results of two logistic regressions corresponding to a full model (with all control variables included) and a restricted model (with nonsignificant controls excluded). The baseline (constant) in this specification represents the 1993 Los Angeles mayoral race. Off-year local elections tend to be characterized by low levels of citizen involvement. Not surprisingly, the 1990 gubernatorial, the 1992 Senate primary, and the 1992 Senate general elections all registered higher turnout.

The individual difference factor with the greatest ability to discriminate between likely and unlikely voters, as expected, was prior voting history. Participants who reported voting in the 1988 election were much more likely to be classified as likely voters in 1992–93 than those who reported not having voted. Partisans, those with higher levels of political interest, the more educated, and whites were also charac-

TABLE 1

Logistic Regression Estimates of the Effect of Tone on Intentions To Vote in the 1990, 1992, and 1993 Experiments

VARIABLE	MODEL FULL	MODEL RESTRICTED
Constant	−.212 (.331)	−.230 (.331)
Advertising tone[a]	.110 (.055)	.114 (.036)
Experiments		
1990 gubernatorial	.434 (.203)	.477 (.195)
1992 primary	.404 (.179)	.335 (.168)
1992 general election	1.221 (.208)	.778 (.138)
1988 turnout	1.746 (.141)	1.614 (.128)
Follow gov't. affairs	.497 (.059)	.501 (.058)
Independent	−1.112 (.108)	−1.122 (.108)
Same party	−.028 (.087)	—
Same gender	−.033 (.117)	—
Age	.002 (.004)	—
Education	.100 (.068)	.129 (.058)
Female	.034 (.119)	—
White	.346 (.131)	.353 (.128)
Log likelihood	−905.5	−906.7
% correctly predicted	78.2	78.1

Note: Entries are logit coefficients with standard errors in parentheses. N = 1,655.
[a] Coded 1 for positive ad, 0 for control ad, −1 for negative ad.

terized by significantly higher levels of voting intention.

From our perspective, the most important result in Table 1 is the effect of advertising tone on voting intentions. In both equations, a one-sided t-test showed that advertising tone significantly (at the .05 level) affected turnout. Converting the logistic coefficient on advertising tone into a linear probability shows that those participants exposed to the negative version of the advertisement were 2.5% less likely to vote than those exposed to no political advertisement. Conversely, the positive version of the advertisement increased voting intention by 2.5 percentage points. In short, the initial estimate of the demobilization effect survived the multiple controls.

Overall, the experimental results demonstrate that exposure to negative (as opposed to positive) advertising depresses intention to vote by 5%. Considering the scope of our experimental manipulation (a single 30-second advertisement embedded in a 15-minute newscast) and the variety of campaigns examined, these effects seem remarkable. Despite our best efforts at experimental realism, it is possible that the effect has been magnified by some aspect of the research design. It is important, therefore, to place the experimental findings in the context of the world of actual campaigns.

Replicating the Experimental Results

To reconstruct our experimental framework in the real world, we measured the tone of the campaign in each of the 34 states holding a Senate election in 1992. Senate campaigns are especially appropriate for our purposes because the candidates rely heavily on advertising (Ansolabehere and Gerber 1993). Moreover, four of our seven experiments focused on Senate campaigns.

Our indicator of campaign tone was based on a systematic content analysis of news coverage of the various Senate races. We searched through the NEXIS and DATATIMES data bases for all newspaper and newsmagazine articles bearing on the Senate campaigns in general and the candidates' advertisements in particular. This search yielded a total of over 2,500 articles ranging from a high of 1,000 on the Feinstein–Seymour contest in California to a low of 28 in the case of the Idaho race. Based on a reading of the news coverage, campaigns were classified into one of three categories: generally positive in tone (scored 1); mixed (scored 0); and generally negative in tone (scored −1). The classification scheme is described in Appendix B, along with each state's tone score.

As our indicator of turnout, we simply computed the votes cast for U.S. Senate and divided by the state's voting-age population. In addition to turnout, we also examined ballot *roll-off* in the Senate elections. For each relevant state, we subtracted the total number of votes cast for senator from the total cast for president and divided by the latter. The roll-off indicator has two distinct advantages. First, roll-off is a campaign-specific effect indicating the degree to which people who were sufficiently motivated to vote in the presidential election chose to abstain in the Senate race. Second, because roll-off uses the presidential vote as a baseline, it adjusts for a variety of state-related differences (e.g., demographic factors, political culture and party competition), which affect the level of voting turnout.[8]

Turnout in senatorial elections depends upon a variety of influences in addition to the tone of the campaign. These include the competitiveness of the race, the volume (or "decibel level") of the campaign, and the electorate's sense of civic duty. (For a thorough discussion, see Rosenstone and Hansen 1993). Our measure of the volume of the campaign was the level of campaign spending by incumbents and challengers (measured in logarithms). Competitiveness or closeness was measured by the squared difference between the Republican and Democratic shares of the total vote. Lastly, to incorporate differences in civic duty and other relevant orientations, we also controlled for per capita income, turnout in the 1988 presidential election, percentage college-educated, region (South, non-South), and the census form mail-back rate.[9]

Having compiled the turnout, roll-off, and campaign tone indicators, we proceeded to replicate the experimental results. Table 2 presents the results from the full and restricted multiple regression analyses of turnout and roll-off. Following the analysis of the experimental data, the tone variable was specified as a trichotomy (negative tone = −1, mixed = 0, positive tone = 1). This specification measures the deviation in turnout and roll-off of the positive and negative campaign tone categories from the mixed-tone category.[10]

Do Senate races characterized by relatively negative campaigns have lower turnout and higher roll-off rates than races in which the campaign is more positive in tone? For both turnout and roll-off, we found significant effects of campaign tone. Negative campaigns decreased turnout by 2%. (This also means that positive campaigns boosted turnout by 2%, for a total difference of 4%.) Negative campaigns also increased ballot roll-off by 1.2% and vice-versa. Since the demobilization hypothesis is directional, we resorted to one-tailed tests (i.e., negative campaigns decrease turnout and increase roll-off, while positive campaigns increase turnout and decrease roll-off). The t-statistics for this hypothesis were 3.64 for turnout and −2.26 for roll-off, both significant at the .05 level.[11]

The use of both experimental and nonexperimental methods to measure the very same naturally occurring phenomena is highly unusual in the social sciences. It is even more unusual if both methods yield equivalent results. In our study, the aggregate-level analysis of turnout and roll-out in the 1992 Senate elections and the experimental studies of negative advertising converge: negative campaigns tend to demobilize the electorate.

TABLE 2

Regression Estimates of the Effect of Tone on Turnout and Roff-off in the 1992 Senate Elections

INDEPENDENT VARIABLE	TURNOUT[a]		ROLL-OFF[b]	
	FULL MODEL	RESTRICTED	FULL MODEL	RESTRICTED
Constant	−.294 (.171)	−.295 (.124)	.157 (.173)	.150 (.040)
Campaign tone[c]	.020 (.006)	.021 (.006)	−.011 (.006)	−.012 (.005)
1988 turnout	.550 (.101	.571 (.090)	.046 (.102)	—
Per capita income	.010 (.027)	—	.048 (.027)	.049 (.019)
Mail-back rate	.337 (.149)	.340 (.125)	−.058 (.151)	—
Southern state	.048 (.015)	.047 (.013)	−.014 (.015)	−.016 (.013)
% College-educated	.120 (.099)	.172 (.076)	−.215 (.100)	−.247 (.067)
Log challenger $	.001 (.005)	—	−.011 (.005)	−.010 (.004)
Log incumbent $	.013 (.007)	.011 (.006)	−.004 (.007)	—
Open seat	.011 (.012)	—	−.009 (.012)	—
(Non)closeness	−.053 (.046)	−.068 (.039)	.058 (.046)	.069 (.037)
R^2	.94	.94	.67	.64
SS residuals	.0099	.0102	.101	.0112

Note: Entries are multiple regression coefficients with standard errors in parentheses. N = 34.
[a] Total Votes for Senate Voting-age Population.
[b] (Total Votes for President − Total Votes for Senate)/(Total Votes for President).
[c] 1 = positive tone, 0 = mixed tone, −1 = negative tone.

Psychological Correlates

That attack advertisements discourage people from voting raises questions about the psychological underpinnings of this effect. One possibility is that partisanship mediates the effects of attack advertisements on turnout. It is generally thought that campaign messages resonate especially strongly among supporters or proponents of the source of the message. Campaigns thus have the effect of reinforcing or crystallizing existing partisan loyalties. Extending this argument to attack advertising implies the obverse. That is, the intention to vote among supporters of the candidate airing the negative advertisement will be unchanged, since the message provides no reasons to vote for their candidate. On the other hand, voting intention should be weakened among supporters of the candidate who is attacked, since the message provides these partisans with reasons not to vote for their candidate.

If attack advertisements demobilize on a partisan basis, we should find a significant interaction effect between advertising valence and viewers' party identification. We thus reestimated the logistic regression presented in Table 1, this time including the appropriate interaction (valence × same party). The results revealed that the interaction term was nonsignificant and had the wrong sign.[12] Partisanship does not mediate the demobilizing effects of attack advertising; supporters of the source and target candidates are not affected differently.

An alternative account of the demobilization effect is that attack advertising generates blanket negativity toward both candidates. According to this "plague-on-both-your-houses" explanation, voters not only become more critical of the target of the attack but turn against the sponsor as well (for some evidence of this effect, see Basil, Schooler, and Reeves 1991). We investigated this possibility by examining participants' evaluations of the personal traits of both the sponsor and the target of the attack advertisements.[13] Our results indicated that attack advertisements generally "work." That is, ratings of the target were generally less positive after participants watched the attack. Ratings of the sponsor, however, were generally unaffected, suggesting that participants did not penalize candidates for airing negative messages.[14]

The fact that attack advertisements do not demobilize on a partisan basis and do not induce negativity towards *both* candidates suggests a third explanation for demobilization. Negative advertising may affect voting intent by conveying cues not about the candidates but about the nature of political campaigns and the political influence of ordinary citizens. Perhaps the act of attacking another candidate in a 30-second advertisement denigrates the entire process.

To explore this possibility, we measured our participants' sense of political efficacy—their beliefs in the responsiveness of public officials and electoral institutions to popular will. We used four questions, coded the responses to each as either efficacious (1) or inefficacious (0) and computed the average response to all four items.[15] Among viewers who were exposed to the positive versions of the campaign advertisement, the mean efficacy score was .24. The mean was no different (.23) among participants in the control group. In the case of viewers who were exposed to the negative versions of the advertisement, the mean dropped to .19. The F-statistic from the one-way analysis of variance was significant at the .02 level.[16] In short, exposure to campaign attacks makes voters disenchanted with the business of politics as usual.

In summary, we have considered three possible explanations for the demobilizing effects of attack advertisements: partisan demobilization, a plague-on-both-houses effect, and general cynicism. Our evidence points toward the third. Among our experimental participants, exposure to attack advertising significantly weakened confidence in the responsiveness of electoral institutions and public officials. As campaigns become more negative and cynical, so does the electorate.

CONCLUSION

Taken together, our studies demonstrate that attack advertising extracts a toll on electoral participation. In the experiments, voting intention dropped by 5% when participants were shown an attack advertisement in place of a positive advertisement. Our aggregate-level replication of the experimental results suggests that Senate turnout in 1992 was roughly 4% lower when the candidates waged relatively negative campaigns. Since the scope of the experimental manipulations never exceeded a single advertisement, our estimates of the demobilizing effects of campaign attacks may be conservative. Over the course of two or three weeks of sustained negative advertising, the flight of voters can be more substantial.

The effects of attack advertising on the decision to vote have significant implications for our understanding of the impact of campaigns on electoral outcomes. Voter withdrawal in response to negative advertising also raises questions concerning the legitimate and fair uses of broadcast advertising.[17]

The most important implication of these results is that in the era of media campaigns, *both* surges and declines in turnout can be generated by high-intensity campaigns. Candidates with sufficient resources can, through the use of negative messages, keep voters away from the polls. Campaigns are not inherently mobilizing forces, and the secular decline in presidential and midterm voter turnout since 1960 (for evidence, see Rosenstone and Hansen 1993) may be attributed, in part, to the increasingly negative tone of national campaigns.

Finally, this research raises normative questions concerning the trade-off between the right to political expression and the right to vote. Should candidates be free to use advertising techniques that have the effect of reducing levels of voter turnout? In the case of publicly financed presidential campaigns, is it legitimate for candidates to use public funds in ways that are likely to discourage voting? How do we weigh the public interest in free political expression against the competing public interest in widespread public participation? When, if ever, should politicians' expression be restrained or subjected to incentives to modify its form or content?

In other areas of public communication, allegations of "antisocial" effects have prompted extensive analysis and debate. In some areas, the outcome has been governmental regulation. Thus the tobacco companies have been banned from using the airwaves for certain forms of commercial speech and are required to include mandated health warnings in their print advertisements. Direct regulation of political speech, which is at the core of the values protected by the First Amendment, is probably both impossible as a matter of law and undesirable as a matter of policy. The classic remedy in this society for injurious speech is simply "more speech." However, there is precedent in the law governing the broadcast media requiring that "equal time" be given to the targets of certain "personal attacks" (see Ferris and Leahy 1990). Possibly, new regulations governing the broadcast media ensuring that the targets of attack advertisements have reasonable opportunity to respond (regardless of their own financial resources) should be considered. Ohio and other states are currently experimenting with "truth in political advertising" guidelines designed to make candidates think twice before resorting to false or misleading advertising. However, approaches that simply ensure that there will be "more speech" miss the essential point raised by this research, which is that negative advertising impacts adversely on voting; remedies that can only multiply the number of negative advertisements will exacerbate, rather than address, the essential problem.

The more realistic approach to influencing the tone of campaign advertising rests on voluntary or incentive-based restraints. There have been several instances in which public controversy over the content of entertainment programming has prodded the networks, local stations, or record companies to withdraw the program in question. Similar reasoning is embodied in legislation pending in Congress that seeks to reform campaign advertising. (For a discussion of recent legislative efforts, see O'Neill 1992). One bill would impose a double standard on advertising rates under which only "positive" advertisements would be entitled to the "lowest unit rate" rule. Other suggestions include the so-called in-person rule, under which the candidates would be required to deliver their attack statements in person (on camera).

A third set of suggestions for reform addresses the use of media "monitoring." In the aftermath of the 1988 campaign, the press decided to scrutinize the candidates' paid messages (in the form of "ad watches," "truth boxes," and the like). Anticipation of critical news coverage may deter candidates from relying heavily on attack messages. To this point, however, there is no reliable evidence concerning the effects of these monitoring effects on voter response to advertising.

We do not yet understand the implications of these various approaches. Some would certainly raise objections from civil libertarians, others would be objectionable to those concerned with political competitiveness. As in the case of campaign finance reform, broadcast advertising reform may work to benefit those in office at the expense of challengers. Although providing incentives for campaigns to air "positive" messages provides no assurance that these messages will be more substantive, verifiable, or honest, they would, at least, be less likely to deter voting. While the case for broadcast advertising reform has yet to be made, the relationship between negative advertising and voting suggests that these issues are worth further research and discussion.

APPENDIX A: SAMPLES OF EXPERIMENTAL ADVERTISEMENTS

The wording and visuals used in two of our studies appear below. The changes associated with the negative versions of the advertisements are given in parentheses.

1990 Gubernatorial Study: Crime

Text. It's happening right now in your neighborhood. A generation of youth slowly dying. ______ is (is not) the candidate who intends to stop this tragedy and preserve California's future. As mayor of __________ , ________ added (reduced the number of) police officers, constructed (blocked) new jails, and fought hard against drugs (opposed drug education programs). The result: major crime rates fell (increased) by 12%. His (her) record won the endorsement (was condemned by) the California Association of Police Chiefs. They know __________ will push for (will oppose) tougher sentencing and strengthen (weaken) our state's justice system.

Visuals. Schoolchildren on playground; addict injecting heroin; body bag being removed from crime scene; police officers outside courthouse; interior of prison cell; candidate logo.

1992 Senate Study: Unemployment

Text. Since 1990, California has lost two-and-a-half million jobs. The state now has the highest unemployment rate in the nation. California needs elected officials who will end the recession. __________ will work (has done nothing) to bring jobs back to our state. As a U.S. Senator, __________ will introduce legislation (__________ opposes legislation) to increase funding for job training programs and to give California companies incentives to modernize and expand their factories and plants. California needs (can't afford) __________ in the U.S. Senate.

Visuals. Closed factory; graph showing state's unemployment rate; lines at unemployment office; picture of candidate (opponent); factory workers assembling planes; workers on construction site; candidate logo.

APPENDIX B: CLASSIFICATION OF 1992 SENATE CAMPAIGNS ACCORDING TO ADVERTISING TONE

We examined a subset of the NEXIS and DATATIMES data bases that contained full-text reproductions of articles from more than 30 major daily newspapers and five politically oriented magazines (e.g., the *Cook Political Report*, the *Roll Call*, and the *Hotline*). A separate search was conducted for all 34 senatorial campaigns. The search was designed to access all articles about the campaign printed after the primary and before the general election. When the search produced more than 150 articles (as was the case for seven races), than a further search command focusing on campaign advertising was added. This procedure elicited a total of 2,573 articles.

Each article was read by a graduate student coder who specifically looked for discussion of campaign tone. The coder followed a strict scheme in order to place each race into one of the three campaign-tone categories. If a majority of the tone-related references to a campaign were negative (e.g., it was characterized as being nasty, dirty, or vicious and provided specific examples of negative attacks from each of the race's candidates), the race was coded as negative. If at least three articles specifically mentioned that one of the Senate candidate's was deliberately refraining from making a negative response to the opponent's attacks and no later article contradicted this information, then the race was coded as mixed. Finally, when the news coverage yielded no discernible information about negative campaign tone, the race was coded as positive.

We validated our news-based classification scheme by asking two major political consultants (David Hill, Republican, and Mark Mellman, Democrat) to rate each of the Senate campaigns on the same three-point scale. The consultants disagreed with our classification in only one instance (Kentucky), and we deferred to their expertise. (The analysis is unchanged if

Kentucky is eliminated from the analysis.) Each state's tone score is shown in the following list:

Negative Tone

Arkansas
California (6-yr. seat)
California (2-yr. seat)
Colorado
Connecticut
Georgia
Indiana
Kentucky
Louisiana
New Hampshire
New York
North Carolina
Ohio
Oregon
Pennsylvania
South Carolina
Washington

Mixed Tone

Alabama
Arizona
Florida
Illinois
Missouri
Oklahoma

Positive Tone

Alaska
Hawaii
Idaho
Iowa
Kansas
Maryland
Nevada
North Dakota
South Dakota
Utah
Vermont
Wisconsin

(Note that the California 6-year seat was contested between Boxer and Herschenson and the 2-year seat, between Feinstein and Seymour, and that the Louisiana general election was uncontested.)

Notes

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1. We use these terms interchangeably to describe whether the advertisement, or the campaign as a whole, focuses on a candidate's positive aspects or on the opponent's liabilities and faults.

2. In addition to minimizing the visual differences in the advertisement, we also used identical logos, in which the sponsoring candidate's name appeared in large red letters against a brown backdrop.

3. Using a weighted average of Los Angeles and Orange counties as the baseline, the demographics for our sample match closely for age (median of 34 versus 31), gender (44% vs. 51% male), race (52% vs. 47% white), and partisanship (49% identifying Democratic vs. 47% registration). Our participants deviated in the local area in two respects: 26% of our participants were African-American (compared to 10% in Los Angeles and Orange counties), and 44% were college graduates (compared to 24% for the local area).

4. Random assignment of participants to experimental conditions was used throughout. The use of random assignment assures (subject to the rules of probability) that differences in the dependent variable can be attributed only to the experimental manipulation (see Campbell and Stanley 1969, 25). We took the additional precaution of controlling for a number of background variables considered predictive of participation, including partisanship, prior voting history, age, and education.

5. Responses to the two questions were generally cumulative; that is, few people who said they were not registered indicated an intention to vote. These respondents were classified as unlikely voters.

6. For a more detailed analysis of the effects of advertising valence in specific campaigns, see Ansolabehere, Iyengar, and Valentino 1993.

7. The number of cases was 1,716. Since the F-statistic is nondirectional, that is a conservative test of the demobilization hypothesis.

8. The average roll-off in the 1992 Senate elections (rounded to the nearest thousand) was 127,000, with a maximum of 609,000 and a minimum of −13,000. In percentage terms, roll-off averaged 4.9% with a range of −1.8% to 11.6%.

9. The Bureau of the Census mails forms to every resident in each state. The mail-back rate is the fraction of forms that are completed and returned.

10. A simple F-test revealed that the symmetry restriction was justified.

11. There are a variety of other interesting results in this analysis, but since we are especially interested in the effects of campaign tone, we set them aside for future consideration.

12. These results are available from the authors.

13. Participants rated the candidates' intelligence, honesty, compassion, leadership, toughness, arrogance, weakness, and deviousness. We formed an index by subtracting the number of negative ratings from the number of positive ratings.

14. These results are available from the authors.

15. The questions asked participants to agree or disagree with the following statements: (1) "Generally speaking, those who get elected to pubic office keep in touch with the people in their constituencies"; (2) "In this country, politics works for the benefits of a few special interests, rather than the public good;" (3) "Most politicians are willing to tackle the real problems facing America;" (4) "Having elections makes government responsive to the views of the people."

16. The F-statistic was 4.0, with 1,716 cases.

17. It is possible, of course, that negative advertising also exerts prosocial effects not tapped by our studies. For instance, there is some evidence that negative advertisements allow voters to differentiate more readily between candidates' issue positions, thus facilitating "issue voting" (see Garramone et al. 1990; Patterson and McClure 1976).

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RIDING WAVES OR MAKING WAVES? THE SERVICES AND THE U.S. DEFENSE BUDGET, 1981-1993

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Bureaucratic politics is the favored explanation of those addressing the perversities of defense budgeting. But it is arguably devoid of politics, given its dependence on either aggregate top-down or horizontal models. I seek to redirect analysis. I disaggregate defense spending (by service and weapon type) and study budget sensitivity to program pressures in the buildups and builddowns of the Reagan–Bush eras. Applying a two-equation model to time-series cross-sectional data, the analysis shows weapon budgets increasing with program diversification and a commitment to defense spending. In turn, it shows programs diversifying to accomodate service objectives: when turning to missions, the services increased program varieties while concentrating program resources.

Bureaucratic politics is the favored explanation of those who ponder the perversities of weapon acquisition and defense budgeting. The idea that weapons and budgets are outcomes of internal governmental conflict prompted numerous studies detailing the competitive strategy and tactics involved (Armacost 1969; Evangelista 1988; Halperin 1974; Hampson 1989; Huntington 1961; Kotz 1988; McNaugher 1989). These studies are complemented by valuable statistical treatments (Majeski 1983; Marra 1985; Mintz 1988, 1989; Nincic and Cusack 1979; Ostrom 1978; Ostrom and Marra 1986). Despite their important contribution, the latter insufficiently capture the insights of the nonstatistical literature. Most often, statistical studies reveal budgets responding to prior-year amounts or to amounts proposed by other actors in the budgeting process. As a result, they subordinate bureaucratic politics to budgeting "incrementalism" (or "inertia") when not confusing the two. They thereby focus on outcomes remote from their underlying political causes within the defense bureaucracy. Statistical studies certainly acknowledge the disaggregated dynamics of defense budgeting; yet even then, they do so incompletely. For example, the military services and/or the Department of Defense are routinely treated as a single decision maker in a process that is implied to be either "horizontal" or "top-down" (on the latter, see Su, Kamlet, and Mowery 1993).

I seek to redirect analysis by offering a model that is open to both top-down and bottom-up influences. It disaggregates defense spending by service and weapon type and studies budgetary sensitivity to program pressures in the Reagan–Bush era—a period of an unprecedented buildup and less striking builddown.[1] By assessing program concentration (and its opposite, diversification), it asks whether the services *responded* to higher budgets or instead *actively created* the budgetary realities of the period. Indeed, the latter is suggested by the bureaucratic politics literature, where the services are depicted as seeking versatile and diverse weapons to claim missions and resources—a quest well within service capability given the service role in the lengthy and complex defense planning, programming, and budgeting cycle (see Hampson 1989, 6–27; Puritano 1985). More explicitly, in examining Reagan–Bush defense spending, this study asks whether service programming followed defense budgeting or vice versa, that is, whether budgetary increases (decreases) caused program expansion (contraction) or the reverse.

Accordingly, this study tests two equations. The first represents a top-down perspective that assumes a coherent actor (the executive branch) choosing among budgetary categories. The second adopts a bottom-up perspective that recognizes programming pressures exerted by self-interested services. While each equation incorporates the determinants of one level of the decision process (budgeting or programming), each acknowledges the possible influence of the other level. The resulting model is tested with cross-sectional time-series data drawn from presidential budget requests from fiscal years 1981–93 for the three services (air force, navy, and army) and for eight service–weapon categories: air force aircraft and missiles; navy aircraft, missiles, and ships; and army aircraft, missiles, and combat vehicles.

BUREAUCRATIC POLITICS

Bureaucratic politics has proven a fruitful perspective from which to understand the outcomes and deficiencies of the acquisition process. It has generated a literature rich in insights and examples. However, it is marred by conceptual deficiencies that limit its explanatory and predictive utility.

Controlling Acquisition

According to numerous intensive studies of defense budgeting and acquisition, bureaucratic politics dominates acquisition throughout, as weapons are initiated and shaped through rivalry. The military services and subservices are key acquisitional competitors. They use weapons to stake claims, to acquire resources, and to affirm key missions. (In this, Pentagon officials, the White House, and defense contracting firms also compete even if they are not, strictly speaking, "bureaucratic" competitors.) The services bargain with other participants, form coalitions, control information, and manipulate the acquisition process to obtain key weapons. They engineer threats, strategically choose requirements, limit oversight, select and distort data, and skip and collapse acquisition stages to create political momentum and proscribe less desired alternatives.

Weapon requirements (or conceptions of "need") are especially critical to political control and follow from overlapping and changing service missions. The air force, navy, and army pursue weapons that serve essential missions: combat missions, controlling the seas, and conducting ground warfare, respectively. And they engage in politics, reinterpreting missions to match new technology or redirecting old technology to new purposes. As service instruments, requirements are animated by the complex relationship between missions and weapon technology and performance. Politics pervades weapon requirements, because any mission can be served by many different means and any means can serve many different missions. That is, bureaucratic politics thrives on weapon *substitution*. A service can use the incomplete fit between mission and technology to advantage, but so can rivals.

Bureaucratic competition partly occurs over single programs. Such programs are valued for key performance yet are made versatile to claim other missions or to deflect claims of other services. Single weapons can expand or contract performances to serve bureaucratic interests in three major ways. First, requirements defer to less critical missions to protect or promote more crucial ones. For this reason, during the 1980s, the air force sought multipurpose interdiction aircraft in the guise of close air support (CAS) by offering an attack version of the F-16 (the A-16) to replace an aircraft specifically designed for close air support (the A-10). Resisting army pressure to acquire dedicated CAS aircraft, the air force offered improved CAS performance largely as a byproduct of general aircraft capability enhancement. Old missions were even offered in the guise of new ones. In the 1990s, navy submariners envisioned changed missions for attack submarines—from deep-water to the shallow-water antisubmarine contingencies of regional conflict, but also from antisubmarine to such land-oriented functions as performing cruise missile attacks, engaging in intelligence gathering, transporting special forces, and undertaking the symbolic functions long performed by battleships and aircraft carriers.[2]

Second, requirements are written to encroach surreptitiously upon rival missions. In the 1950s, the army disguised its effort to acquire an intermediate-range ballistic missile capability. Dissatisfied with the 1947 formalization, "Armies walk, navies sail, and air forces fly," the army sought to acquire ballistic missiles (the Jupiter) by emphasizing the link between the performance of missiles and artillery. Of course, the air force defended against these encroachments by associating longer-range missiles with its strategic mission. By achieving restrictions on the range of army tactical ballistic missiles, it successfully fought off army attempts to place a technological "foot in the door" (Armacost 1969).

Third, requirements are supported or added when program alternatives are proscribed (or killed). When, during the late 1960s and early 1970s, limits were imposed on launchers (first by McNamara and then by SALT), the air force turned to multiple warheads to improve target coverage and force survival. But all the services sought flexible weapons with limited budgets and infrequent program startups. In the 1980s and 1990s, weapons such as the air force B-2 stealth bomber and air force advanced technical fighter (ATF), the army light (stealth) helicopter and M1 Abrams tank, and the navy SSN-21 attack submarine were deemed especially useful in increasing political support and ensuring adequate mission coverage.

Bureaucratic competition also yields multiple programs. Program numbers expand or contract to serve bureaucratic interests in four basic ways. First, requirements are written creating new weapons to protect other favored weapons. These requirements can be designed to ensure that the cost of one project does not endanger another. In the 1980s, the financial health of the advanced tactical fighter program, for instance, certainly moved the air force to avoid entirely new CAS aircraft. Requirements can also be designed to maintain a division of labor among weapons to ensure that projects do not threaten the rationale for more desirable ones by offering the same mission at a lower price. In the 1990s, air force advance tactical fighter proponents supported designs for an F-16 replacement that tilted toward the air-to-ground (rather than the competitive air-to-air) mission and then advocated that new designs be shelved in favor of a somewhat upgraded F-16 (Morrocco 1992, 34). Moreover, deficiencies in requirements are addressed by promoting companion weapon systems. For instance, the navy answered criticisms of aircraft carrier vulnerability by stressing the defensive layers that would have to be penetrated by an attacker.

Second, requirements are written to avoid undesirable or controversial missions and thus to create a rationale for multiple systems. The navy, for instance, spurned air force tactical aircraft and missions and, in the late 1980s, failed to embrace the ATF. Both services desired an air-superiority fighter, but each rejected the other's missions: the air force pursued interdiction and the navy sought carrier compatability, air defense, and ground support (United States, General Accounting Office 1990).

Third, requirements are formulated to promote multiple programs, regardless of overlaps. Multiplicity reduces political risk, avoids choices that could alienate bureaucratic factions, and increases political leverage to secure the survival of at least one program. In the 1970s, elected officials justified opposing one weapon (the B-1) by supporting another (cruise missiles or the stealth bomber) (Kotz 1988, 153). Politically, critics cannot easily oppose multiple service programs. President Carter apparently endorsed the MX missile because he opposed the B-1 bomber (pp. 167–72); and Henry Kissinger apparently accepted the Trident and cruise missiles to acquire the support of the joint chiefs of staff for the SALT I agreement (Hampson 1989, 189). In any case, multiplicity increased the service claim to available resources.

Finally, requirements are written to kill a project, especially when a joint-service one, creating a need for single-service weapons. Demonstrating a subtle, passive variant of this tactic, the air force and navy might have sabotaged the joint advanced tactical fighter (ATF/NATF) program by allowing the aircraft to evolve away from navy requirements: the air force was to commit to a contractor (in 1991) for full-scale development when the navy was only completing concept demonstration/validation.

Bureaucratic politics thus centers on weapon substitution, as actors protect and promote immediate interests while constructing supportive political coalitions and combating opposing ones.[3] In this environment, players can direct requirements toward single yet versatile weapons made to perform in "essential" ways though able to perform multiple missions or toward multiple weapons made to perform newly defined or more broadly defined missions. (Only the latter is the subject of the present analysis.)[4]

So directed, service program decisions can place upward pressure on the procurement budget. By diversifying programs, the services can claim future resources. Once commiting procurement funds to a program, the services can argue for more funding to reverse the uneconomic production rates associated with program "stretch-outs" (services sink costs in production, then argue for more funds to reduce production inefficiencies), to keep assembly lines warm, to capitalize on the learning curve possible with large production contracts, to correct for intentionally underestimated procurement costs, to meet the increased budgetary demands of previously deferred procurement (in the 1980s, the budgetary "bow wave"), or to procure weapons linked to existing ones (tankers to refuel bombers, precision-guided weapons to serve aircraft, attack aircraft to serve aircraft carriers, etc.).

In this, Congress is variously portrayed as an active or passive accomplice. Like the services, legislators are commonly assumed to be actively self-interested: they promote programs with constituent benefits, since defense dollars mean jobs and profits and jobs and profits mean votes and campaign support. Yet while "simple, parsimonious, and persuasive," pork-barrel explanations "are also mostly wrong" (Mayer 1991, 210). Statistical analyses are rarely as exhaustive or conclusive as on this point: constituent contract benefits do not predict votes in either the House or the Senate (see, e.g., Lindsay 1990). These analyses are backed by the indirect evidence that Congress rarely increases funding over Pentagon-requested levels. Indeed, Congress is not a highly diligent or effective acquisition manager, despite charges of excessive congressional interference in program management ("micromanagement"). The Congress generally defers on matters of acquisition policy; and when intervening, it acts through symbolic stands and informational requests that do not restrict legislative or executive options. It largely accepts the number and variety of programs contained within Defense Department requests; and when recommending changes, it does so partly to keep program funding within the overall budget. By the time Congress gets involved, options are already screened and foreclosed, and programs are too far along to be canceled. Possessing limited resources, expertise, and access, informed only after important decisions are made, and relying on dated and incomplete information, Congress cannot effectively influence crucial decisions on weapon performance, source selection, and cost.

Legislative constraints are not lost on the services. Hardly naive participants in weapon acquisition, the services establish requirements to promote missions and suppress the costs of doing so. For as long as possible, they perpetuate the myth that a weapon is required, on schedule, and is progressing as planned. The day of reckoning might come when requirements are found to be overstated and performance and/or cost targets are shown not to have been met. At that point, the political package can unravel. Critics will join forces, supportive alliances will form again, and new political opportunities for intervention can arise. Still, weapon proponents can prevail. When a financial commitment is made to a weapon, it becomes less costly than alternatives, and—ironically—alternatives can be hurt by the fear that any new program, like the current one, will develop beyond control.

Unanswered Questions

Despite the compelling image of the services as political players, the bureaucratic politics perspective leaves important questions unanswered. The bureaucratic politics perspective fails to address whether the services, in diversifying, respond more to budgetary opportunities (the top-down perspective) or to their own technological capabilities and mission objectives (a bottom-up perspective).[5] Ironically, if bureaucratic politics remains the conventional explanation for the perversities of acquisition, it is because it embraces three ambiguous assumptions.

First, the bureaucratic politics perspective focuses on single goals (missions or interests) and neglects trade-offs and choices among objectives. It therefore understates the complexity of decision making by failing to ask whether mission and interests can conflict and, if so, under what circumstances and with what consequences. In not exploring the relationship between missions and objectives, bureaucratic politics fails to recognize that "missions" and "interests" can be incompatible: sometimes key mission performance can be purchased only with a severe price—as when weapons perform below expectation, perform inappropriately for actual missions, or drain scarce resources. Bureaucratic politics thus fails to establish under what circumstances the services translate missions into weaponry and what costs the services willingly incur.

Second, the bureaucratic politics perspective sometimes relies on assumed "uncertainty." Attributing decisional lapses (performance failings, cost overruns, etc.) to incomplete information (rather than irrationality), theorists can retain the core rationality assumptions of bureaucratic politics. Nevertheless, the bureaucratic politics perspective was not originally developed around sophisticated understandings of uncertainty and risk. Thus it does not indicate whether players are best described as risk-adverse or risk-taking. It also leaves open the possibility that uncertainty actually reflects nonrational thinking rather than informational ambiguities, gaps, and contradictions in policymaking, planning, and analysis. In any case, uncertainty can itself undermine rationality, given the problems that decision makers have in estimating and analyzing probabilities—problems that could lead them to ignore risk entirely.[6] Bureaucratic politics thus fails to establish whether and when the services act conservatively in response to the unknowns of high-technology acquisition.

Third (and most relevant to this inquiry), the bureaucratic politics perspective generally ignores the motives of participants. It is ambiguous about whether actors should be viewed primarily as conflictual or cooperative, aggressive or passive, expansionist or preservationist. Sometimes the services are shown coveting the missions of others, unable to live by agreements, and seeking to increase their resource share. At other times, the services are shown rejecting new missions, desiring to compromise, and willing to accept their traditional share of the resource pie. Bureaucratic politics does not reveal the conditions under which the services seek to extend gains (an "active" stance) or to preserve them (a "reactive" stance). For one thing, it does not indicate whether the services measure gain in absolute or relative terms: if the former, the services could be satisfied with gains despite those of service rivals; if the latter, the services could always be dissatisfied, especially if all strive for superiority. Nor does bureaucratic politics reveal the conditions under which the services seek to maintain existing missions (an "inward" orientation) or to augment them (an "outward" orientation). As Figure 1 shows, stance does not determine orientation: if active, the services can be territorial or imperialistic; if reactive, the services can conservatively resist change or acquire new missions when protecting older ones or handed opportunities (filling a political vacuum).

The bureaucratic perspective thus requires significant elaboration. It must disclose *why* and *when* the services extend or contract commitments[7]—that is, whether and under what conditions the services react to budgetary opportunities or instead create budgetary realities. In other words, it must reveal whether bureaucracies have distinct personalities (or motives) and under what conditions these personalities are revealed.

FIGURE 1

Four Different Bureaucratic Personality Types

		ORIENTATION	
		Inward	Outward
STANCE	Active	Territorial	Imperialistic
	Passive	Conservative Caretaker	Opportunistic Defensive

Seeking insights into bureaucratic motives, I shall examine the four different "personality types" presented in Figure 1. These types are based on two interconnected dichotomous dimensions—stance (active/passive) and orientation (inward/outward)—that can be empirically measured. Accordingly, as shown in Figure 2, an active stance is assumed to prevail when service program decisions determine the weapon budget, and a passive stance is assumed to prevail when the opposite is true. An inward orientation is assumed to prevail when programs contract with larger budgets, an outward orientation, when the reverse. (Here, orientation represents movement toward or away from concentration in single programs—presumably those critically tied to the "essential" service mission [see Halperin 1974]—for beyond preferring combat platforms, the navy pursued control of the seas as a *carrier* mission; the air force conceived strategic "bombing" as a long-range, nuclear (and now also conventional) *bomber* mission; and the army organized its missions around the *tank* and now attack *helicopters*.)

THE MODEL

I shall examine procurement at two levels, budgetary and programmatic. The budgetary level assumes a decision hierarchy, where macro budgetary choices inform programmatic decisions. The programmatic level assesses program choices from below as an upward pressure on service budgets. Yet importantly, each level is open to the potential effects of the other. The budgetary level reflects overall levels of civilian budgetary support (from the White House or Department of Defense) for a service and/or weapon

FIGURE 2

Expected Results for Different Bureaucratic Personality Types

		ORIENTATION	
		Inward	Outward
STANCE	Active	PROGRAMMING Territorial POSITIVE	PROGRAMMING Imperialistic NEGATIVE
	Passive	BUDGETING Conservative Caretaker POSITIVE	BUDGETING Opportunistic Defensive NEGATIVE

UPPER-LEFT CORNER: Causal priority—programming or budgeting? BOTTOM-RIGHT CORNER: Direction of relationship between programming concentration and budget size—positive or negative?

type and general trade-offs within the defense budget among research-and-development (R&D), operations-and-maintenance (O&M), manpower, and procurement. But the budgetary level is also hypothetically linked to programming pressure so as to determine whether service program tactics can explain what macro allocation decision cannot. In turn, the programmatic level presents program contraction and expansion as service political instruments. The extent of program concentration thus changes with service goals (mission objectives) and available means (technological capabilities). But like the budget, concentration levels could be open to cross-level (or multilevel) influences and are thus assessed also for their responsiveness to the prior-year weapon budgets (i.e., budgetary opportunities).

The two levels yield a model consisting of two (somewhat interactive) equations:[8]

$$\text{WBDGT} = a_0 - b_1\ \text{ACONC}\ (t-1) + b_2\ \text{DEFCOM} - b_3\ \text{OMTOFF} \tag{1}$$

$$\text{ACONC} = a_0 - b_1\ \text{WBDGT}\ (t-1) - b_2\ \text{MISEMP} - b_3\ \text{RDBLNC}, \tag{2}$$

where WBDGT is the service *weapon budget*; ACONC (or UCONC) is program *concentration* levels for program budget *amounts* (or for *unit costs*, controlling for the number of weapons procured); DEFCOM (*defense commitment*) is the service budget for nonweapons and includes R&D, O&M, and "procurement" (other than designated aircraft, missiles, ships, or combat vehicles); OMTOFF is the budgetary *trade-off* between O&M and procurement (a ratio of O&M to the combined O&M and procurement budgets); MISEMP (*mission emphasis*) is the proportion of the weapon budget devoted to combat platforms, and RDBLNC is the *balance* between *R&D* and procurement spending (here, conceived as an interaction term).

The sign of all coefficients in equation 2 and the concentration variable in equation 1 reflect the expansionary effects commonly assumed in the bureaucratic politics literature. These relationships could be signed in the opposite direction to indicate contractive effects (as the discussion will reveal).

Equation 1 assesses aggregate budgets and their responsiveness to executive allocation preferences. For one thing, the equation posits weapon budgets to be positively related to the executive commitment to nonweapon defense spending (DEFCOM). By aggregated a wide range of budget categories, this variable has a number of characteristics that fit with executive commitment: it is insensitive to changes in emphasis within defense budgeting, it includes amounts (e.g., for O&M) that are relatively stable in the period assessed, it captures the effects of scale (among time periods and service budgets) and therefore subsumes variance attributable to "incrementalism," and it is better conceived as explaining—than as explained by—weapon budgets. The equation also suggests that weapon budgets respond to trade-off decisions (OMTOFF) among budgetary categories. Specifically, it tests whether decision makers sought to improve the operational readiness of existing systems or to procure new ones (the latter being the apparent response of the Reagan administration to the "hollow force" of the seventies). While decision makers could also trade-off R&D and procurement, investing in future rather than current technology (the Bush administration's proposed post–Cold War transition plan emphasizing weapon prototyping over producing), the trade-off between O&M and procurement is likely to be more important in the period assessed. Finally, equation 1 defers to cross-level influences, incorporating the rival bureaucratic politics thesis through ACONC (and UCONC)—testing budget sensitivity to prior-year program concentration.

Equation 2 directly assesses programmatic influences by explaining concentration—the spread in program funding. For one thing, it posits that concentration responds to the service desire to expand, through mission emphasis (MISEMP). This variable represents the proportion of the entire service weapon budget devoted to "combat platforms" and "strategic weapons"—for the air force, strategic bombers (the B-1B and B-2), fighters (the F-15), and attack planes (the F-16); for the navy, combat ships (carriers, cruisers, destroyers, and frigates), fighters (the F-14), and attack planes (the F/A-18); and for the army, attack helicopters (the AH-64 Apache) and tracked combat vehicles (the Abrams M-1 tank, the Bradley Fighting Vehicle, etc.).[9] These systems, crucial to service missions, are assumed to be emphasized at the times when the services choose to expand or to contract the service role (thereby enlarging or narrowing the service–weapon mix). Indeed, in these defining periods, the services might do both—expand program variety measured in unit costs (UCONC) while concentrating budgetary resources (ACONC) in more-essential programs.

If mission emphasis captures a service's will to expand, RDBLNC taps a service's capability to produce a large array of technology. It assumes that rather than being traded off against each other, R&D and procurement funding must converge for new technologies to be introduced. In other words, both are assumed to be necessary—and neither is assumed to be sufficient—for program expansion in a high-technology age. Such a balance seems especially likely to explain diversification measured in unit costs (UCONC) (i.e., controlling for the number of weapons procured) as R&D expenditures likely generate programs but are a "sunk cost" that does not increase linearly with increased production. But the R&D/procurement balance could still yield diversification in program budget amounts. Under the prevailing practice of "concurrency," the R&D and production budgets are tied together. Even if weapons are prototyped in full-scale development, development continues into production because systems remain incomplete and continue to be elaborated, modified, or changed to capitalize on unforeseen technology opportunities or to address unforeseen problems from delayed testing, subsystem integration, or production.

Like equation 1, equation 2 tests for effects of the rival thesis by positing a relationship between program concentration (ACONC or UCONC) and the prior-year weapon budget (WBDGT). It thereby acknowledges that relative concentration may respond to available resources rather than determining them, with the services widely distributing available funds (the hypothesized expansionary effect) or else burying them in essential programs (a contractive effect). In all, then, equation 2 tests three sources of expansion: objectives (MISEMP), capability (RDBLNC), and opportunity (WBDGT).[10]

While offering a more dynamic, political view of the budgetary process, the model still makes two simplifying assumptions: (1) it does not assume that the services differ *fundamentally* in their motives but rather that they diverge in objectives, capabilities, and opportunities; (2) it does not account for changes in the political environment (e.g., the opportunities and threats presented by rival service budgets and weapons) but assumes that these changes equally affect the three services (and weapon types) except when influencing through the incentives and constraints modeled here. (For instance, while the army, by nature, seems labor-intensive and hence less likely to expand, it also has a greater R&D–procurement imbalance.)

THE DATA

All model variables are measured using available budgetary data. The resulting indicators show interservice difference over time and intraservice differences across weapon types.

Creating the Indicators

Measures were created from administration budget requests found in congressional hearings for fiscal years 1980–93,[11] including proposed program amounts and quantities and R&D, "procurement," and O&M funding.[12] They were constructed as indicated in the Appendix.[13]

Program concentration is measured by the Herfindahl–Hirschman index (Hirschman 1945), adjusted by the square root (Michaely 1984) to increase sensitivity in midrange. The measure varies between 0 and 1.0, with larger values associated with higher concentration (on concentration measures, see Taagepera 1979). Concentration was calculated for program budget amounts (ACONC) and program unit costs (UCONC), both by service (e.g., air force) and service–weapon type (e.g., air force missiles).[14]

Weapon budget (WBDGT) includes allocations for certain kinds of nongeneric weapons, usually designated by type and model (e.g., the Trident II missile). It includes most air force aircraft and missiles; navy aircraft, missiles, and ships; and army aircraft (helicopters), missiles, and combat vehicles.[15] It excludes support equipment, ammunition, modifications, and some generic systems ("trainers." "decoys," etc.).[16] All of the spending ("other" procurement) was added to R&D and O&M spending to create the nonweapon budget, measuring the defense commitment (DEFCOM).

TABLE 1

Average Budget Amounts, Concentration Levels, and Mission Emphasis, FY1980–93, by Service

	AIR FORCE				NAVY				ARMY			
VARIABLE	80–82	83–86	87–90	91–93	80–82	83–86	87–90	91–93	80–82	83–86	87–90	91–93
Amounts[a]												
Weapon	5.5	19.5	9.6	8.5	13.6	24.2	19.3	12.9	4.2	8.8	5.6	2.2
Other	10.1	20.6	20.6	13.9	2.6	10.4	8.4	9.2	3.5	10.6	8.5	4.8
R&D	7.0	13.4	14.4	11.9	5.1	8.7	8.8	7.1	3.3	4.8	4.8	4.9
O&M	13.7	19.1	19.1	16.6	16.8	23.8	22.7	19.2	12.7	18.2	9.5	6.9
Concentration												
Budget	.43	.42	.42	.45	.28	.28	.26	.35	.39	.34	.39	.35
Unit	.45	.41	.48	.50	.41	.40	.36	.47	.46	.41	.44	.54
Mission emphasis	.73	.68	.56	.54	.65	.60	.63	.65	.54	.48	.56	.22

[a] Amounts are in billions of 1982 dollars

The next two variables rest on standard measures for budgetary trade-offs (see Berry and Lowery 1990). The O&M trade-off assumes a fixed-amount division, with O&M spending occurring at the expense of procurement and vice versa. The trade-off is expressed as a proportion to combined spending for O&M and "other" procurement spending of the difference between the two.[17] The R&D (RDBLNC), while not strictly a trade-off, is mathematically rooted in the trade-off formula, though squared to emphasize instances where R&D and other procurement diverge.

Finally, mission emphasis (MISEMP) is a ratio, to weapon budgets, of combined budgets for combat platforms and strategic weapons, including all strategic nuclear programs (also cruise missiles), attack aircraft (fixed-wing and helicopters), and fighters, navy fighting ships (aircraft carriers, submarines, destroyers, cruisers, and frigates), and army tracked combat vehicles (tanks and fighting vehicles). These budgets exclude allocations for systems such as cargo aircraft and ships, communication, reconnaissance, and electronic-warfare planes, trainers, landing craft, support vehicles, precision-guided (nonballistic and "noncruise") missiles, and so on.

Describing the Indicators

Tables 1 and 2 offer time-series and cross-sectional views of the data, respectively. They reveal difference over time among the services and often greater cross-sectional differences among the services by weapon type.

Table 1 reveals the average service budgets, concentration levels, and mission emphasis for 1980–93 in four consecutive periods (in 1982 dollars).[18] For all three services, the weapon budget rose dramatically into the mid1980s and declined thereafter, entering the 1990s near the levels of the early 1980s. And, while R&D and O&M budgets similarly rise and fall (with the exception of the army R&D budget), only "other" procurement changes as dramatically as the weapon budget; and then, the other budget remains relatively constant until the late 1980s for the air force and into the 1990s for the navy. Only for the army

TABLE 2

Average Budget Amounts, Concentration Levels, and Mission Emphasis, FY1980–93, by Service and Weapon Type

	AIR FORCE		NAVY			ARMY		
VARIABLE	PLANE	MISSILE	PLANE	MISSILE	SHIP	PLANE	MISSILE	VEHICLE
Amounts[a]								
Weapon	8.7	2.6	5.7	3.0	9.5	1.3	1.8	2.4
Other	12.9	4.0	2.5	1.3	4.1	1.8	2.5	3.0
R&D	9.2	2.8	2.4	1.3	3.9	1.1	1.7	1.7
O&M	13.4	4.0	6.6	3.5	10.8	4.3	6.3	6.6
Concentration								
Budget	.53	.56	.46	.47	.46	.71	.48	.69
Unit	.54	.68	.35	.66	.45	.68	.70	.68
Mission emphasis	.65	.55	.55	.48	.72	.69	.02	.73

Note: Nonweapon amounts are estimates based on Amount × (Service Weapon Budget per Type/Service Weapon Budget).
[a] Amounts are in billions of 1982 dollars.

TABLE 3

The Effects of Program Concentration on Weapon Budgets, 1981–93, by Service

	ORDINARY LEAST SQUARES		CORRECTED FOR AUTOCORRELATION & HETEROSCEDASTICITY	
INDEPENDENT VARIABLES	COEFF.	S.E.	COEFF.	S.E.
Amount cencentration (t − 1)	−16,448.26	12,382.23	−32,422.60*	12,711.60
Dummy	—	—	17,021.62*	5,552.12
Defense commitment	.80***	.13	.51**	.14
O&M trade-off	25,686.77***	5,860.95	8,328.24	5,328.05
Intercept	−32,312.70***	6,442.90	−492.30	4,908.73
Army	—	—	−2,398.63	1,932.05
R^2	.60	—	.72	—
F	17.33***	—	16.98***	—
Unit concentration (t − 1)	2,030.58	10,950.42	−5,404.14	7,990.70
Dummy	—	—	−20,175.70***	4,456.11
Defense commitment	.87***	.13	.58***	.12
O&M trade-off	28,492.24***	5,619.69	10,060.41	4,136.76
Intercept	−30,760.10**	8,056.78	−4,580.34	3,212.24
R^2	.58	—	.50	—
F	15.96***	—	8.37***	—

Note: The amount concentration dummy is for the navy, the unit concentration dummy, for the army. N = 39.
*$p \le .05$.
**$p \le .01$.
***$p \le .001$.

O&M budget does an R&D or O&M budget eventually drop below the levels of the early 1980s.

Program concentration is a different story. Unit concentration changes more dramatically than amount concentration over the entire period—for the navy sizably shifting toward concentration from the late 1980s into the 1990s, for the army similarly shifting over a longer period, into the 1990s. Regardless of measure, however, only army amount concentration does not reach its highest level by the early 1990s.

The table shows somewhat different findings for mission emphasis. The services differ in relative preference for combat platforms and strategic weapons and show differing patterns of change over time. The army, not surprisingly, is the least wedded to such systems, especially by the early nineties, when less than a quarter of army weapons were of this type. Furthermore, while the air force reduced its mission emphasis over time, the army vacillated, and the navy apparently persevered (with a fairly constant emphasis over the entire period).

Table 2 offers a cross-sectional look at the data, by service and weapon type. Weapon budgets and concentration levels vary more dramatically across types than over time. Missile budget levels are relatively small for all the services; and they are the smallest budgeted item for the air force and navy. Predictably, the largest budgets are for service-defining systems: air force aircraft, navy ships, and army combat vehicles. Yet these budgetary preferences are only somewhat reflected in concentration. In budgeted amounts, interservice concentration varies more than intraservice concentration, with the exception of the army missile budget (far less concentrated than the army plane and vehicle budgets). The services actually exhibit relatively constant amount concentration across programs, with the army displaying, on average, the highest concentration and the navy, the lowest. Per unit, the pattern is different. Missile costs are highly concentrated in single programs and are the most concentrated parts of the budget; army budgets are evenly concentrated across programs; and the navy aircraft budget is by far the least concentrated, incorporating fighters, attack planes, helicopters, antisubmarine warfare aircraft, combat support planes, and so forth. The R&D, O&M, and other procurement budget estimates show air force aircraft budgets and the navy O&M budget to be among the largest of the services.[19]

The table also suggests that mission emphasis varies by service. Once more, the army boasts by far the lowest mission emphasis value. With an otherwise high mission emphasis, the army devoted an average of only 2% of the service missile budget (accounted for by the intermediate-range Pershing II missile) to strategic weapons (a distribution largely beyond army control). All three services show the greatest mission emphasis in well-budgeted service-defining weapon categories, revealing higher unit costs and the priority given such systems.

In short, the data indicate that the analysis will be sensitive to whether budgets are treated by service or by service and weapon type and to whether concentration is studied by budgeted amounts or by unit costs. They also suggest patterns over time and interservice and intraservice differences that require statistical correctives.

TABLE 4

The Effects of Program Concentration on Weapon Budgets, 1981–93, by Service and Weapon Type

	ORDINARY LEAST SQUARES		CORRECTED FOR AUTOCORRELATION & HETEROSCEDASTICITY	
INDEPENDENT VARIABLES	COEFF.	S.E	COEFF.	S.E.
Amount concentration (t − 1)	−11,503.90***	1,920.98	−11,483.20***	1,452.71
Dummy	—	—	−215.07	1,124.25
Defense commitment	.38***	.03	.32***	.03
O&M trade-off	3,996.57**	1,168.36	3,488.32*	1,155.19
Intercept	3,878.45**	1,179.38	5,127.69***	871.17
Navy planes	—	—	−2,530.23**	701.07
Navy missiles	—	—	−1,805.76*	666.45
Army planes	—	—	−2,433.25**	690.55
Army vehicles	—	—	−3,716.62***	687.05
R^2	.69	—	.78	—
F	75.89***	—	42.39***	—
Unit concentration (t − 1)	−3,921.07*	1,577.59	−3,013.88	1,338.48
Dummy	—	—	−3,294.12*	1,249.18
Defense commitment	.37***	.03	.38***	.04
O&M trade-off	5,029.99**	1,353.01	−292.07	1,006.90
Intercept	−200.90	1,020.70	29.14	699.13
Navy planes	—	—	2,147.53***	466.27
Navy missiles	—	—	1,542.97*	546.08
Navy vehicles	—	—	6,333.02***	700.55
R^2	.61	—	.81	—
F	52.02***	—	58.53***	—

Note: The amount concentration dummy is for air force aircraft, navy ships, and army missiles, the unit concentration dummy, for air force aircraft and navy ships. N = 104.
*$p \leq .05$.
**$p \leq .01$.
***$p \leq .001$.

RESULTS

The analysis employs two different units of analysis and two different budgetary assumptions to provide perspective on observed relationships. The model is analyzed on service annual budgets, 1981–93, yielding 39 cases, and on service budgets per weapon type (service–weapons), yielding 104 cases, with the plausible assumption that the services discriminate by weapon type (e.g., treating aircraft or ships differently from missiles).[20] The model is also analyzed using concentration in program amounts (termed amount or budget concentration) and unit costs (termed unit concentration or program variety)—the latter recognizing that amounts budgeted can obscure actual program variety. Given "foot-in-the-door" tactics, unit concentration (controlling for budgeted amounts and hence the number of weapons procured) could be a more useful measure.

The analysis requires iterative estimating techniques due to the use of time-series cross-sectional (pooled) data. With such data, autocorrelation patterns and residual variance can vary by cross section (by service or service–weapons). For this reason, after correcting for first-order autocorrelation by cross section, the data are merged to obtain full-sample equation estimates. Following Stimson (1985), the residuals are examined and a dummy variable is introduced to generate a separate coefficient for cross sections with higher residual variance: the dummy is set to the value of concentration or the weapon budget (the independent variable) for higher-variance cross sections and zero for the others. Estimates are again obtained and binary dummies are fitted to adjust the intercept for cross-section residuals clustering above or below predicted values; and the procedure is repeated until all cross-section residuals spread evenly around the predicted amounts.[21] The results provide stronger support for equation 1. Yet they also support equation 2 and suggest that programmatic decisions have shaped the weapon budget.

The Weapon Budget Equation

The analysis reveals the importance of programmatic influences on the weapon budget. Yet results vary somewhat by unit of analysis (service or service–weapons) and concentration measure (program amounts or unit costs). Tables 3 and 4 show the findings for equation 1. Whether assessed by service or service–weapon, concentration amounts or unit costs, the tables strongly indicate a positive relationship between defense commitment and the weapon budget, a not entirely expected result given potential budgetary trade-offs.[22] Assessed by service–weapon for amount concentration, the O&M trade-off vari-

TABLE 5

The Effects of Weapon Budgets on Program Concentration, 1981–93, by Service

	ORDINARY LEAST SQUARES		CORRECTED FOR AUTOCORRELATION & HETEROSCEDASTICITY	
VARIABLES	COEFF.	S.E.	COEFF.	S.E.
Amount concentration				
Weapon budget (t − 1)	−.58**	.15	.19	.16
Dummy	—	—	−1.22***	.12
Mission emphasis	13,333.70	7,888.30	9,006.80	6,211.20
R&D balance	−23,111.10	20,509.70	−28,212.50	17,246.00
Intercept	36,743.40***	4,264.70	38,479.80***	3,150.50
Air force	—	—	190.16	2,503.40
R^2	.30	—	.91	—
F	5.02**	—	70.16***	—
Unit concentration				
Weapon budget (t − 1)	−.57**	.13	−1.03***	.23
Dummy	—	—	5.31***	.78
Mission emphasis	−9,366.10	7,098.20	−6,921.30	10,933.50
R&D balance	−55,996.70*	18,455.60	−114,118.90*	34,952.50
Intercept	58,831.70***	3,837.50	58,013.90***	6,703.60
Air force	—	—	7,163.70	3,084.10
R^2	.49	—	.78	—
F	11.00***	—	24.01***	—

Note: The amount concentration dummy is for the navy, the unit concentration dummy, for the army. All coefficients are multiplied by 10^5. N = 39.
*$p \le .05$.
**$p \le .01$.
***$p \le .001$.

able is also significant, though not in the predicted direction. Apart from the relationship between the weapon budget and defense commitment, O&M budget increases translate into weapon procurement. The Reagan years were apparently a true "time of plenty" in which defense trade-offs—between weapons and readiness or even present and future capability—were spurned. (Preliminary analyses showed little evidence of a trade-off between R&D and procurement spending.) Nevertheless, when assessed by service or service–weapons, or concentration by program amount or unit costs, prior-year program concentration significantly explains the weapon budget, a negative sign indicating that program expansion exerted upward budgetary pressure. (When assessing by service, program concentration is negatively related to budget amounts for all three services since the navy, represented by the dummy variable, exhibits a negative sign when the concentration and dummy coefficients are added.) The concentration coefficient is significant in the analysis by budgeted amount though not by unit cost; but for unit cost, the dummy variables (representing the army in the service analysis and air force aircraft and navy ships in the service–weapon analysis) are negative and significant. In all, the analysis suggests an expansionary programmatic influence on the weapon budget. (Indeed, the findings for budget concentration, on the service–weapon sample, are fairly robust and persist regardless of estimating technique, including corrections for autocorrelation alone.)[23]

The Program Concentration Equation

The program concentration analysis yields more mixed results, varying by sample and concentration measure. As revealed in Table 5 and 6, program concentration is sensitive to both programmatic and budget-level influences. The analysis produced a number of insignificant coefficients. Still, in the service analysis, the R&D–procurement balance significantly predicts program expansion and, as expected, the conjoining of R&D and procurement spending leads to program proliferation (in unit concentration) rather than a spreading of budgetary resources (in amount concentration). Also, as expected, in both service–weapon estimates, mission emphasis is a significant determinant,[24] associated with lower concentration in unit costs and higher concentration in program amounts. These findings reveal that in turning to mission-defining systems, the services concentrated program budgets in fewer systems (apparently the "most essential" ones: strategic bombers, aircraft carriers, etc.) even as they diversified the service arsenal.

The findings for the effects of the prior-year weapon budget are more complex. In the service analysis, the navy is shown diversifying in response to prior weapon budgets, both by programmed amounts and unit cost, while the air force is shown diversifying by unit cost. But the army pattern is different: in the service–weapon analysis, the army, too, diversified unit costs with increased budgets; yet

TABLE 6

The Effects of Weapon Budgets on Program Concentration, 1981–93, by Service and Weapon Type

VARIABLES	ORDINARY LEAST SQUARES		CORRECTED FOR AUTOCORRELATION & HETEROSCEDASTICITY	
	COEFF.	S.E.	COEFF.	S.E.
Amount concentration				
Weapon budget (t − 1)	−1.42***	.29	.07	.31
Dummy	—	—	−.68	.85
Mission emphasis	15,754.50***	3,995.20	24,617.70**	6,481.60
R&D balance	5,968.10	21,005.90	991.60	25,361.30
Intercept	52,064.50***	2,682.30	43,536.40***	4,415.80
Air force planes	—	—	−7,981.50*	2,811.80
Navy ships	—	—	−22,466.30***	2,828.50
R^2	.26	—	.66	—
F	11.45***	—	19.03***	—
Unit concentration				
Weapon budget (t − 1)	−2.13***	.39	−.49	.55
Dummy	—	—	−6.24**	1.63
Mission emphasis	−6,992.80	5,467.20	−16,914.50**	4,644.00
R&D balance	−18,724.20	28,745.10	35,964.00	28,049.20
Intercept	73,389.20***	3,670.60	91,113.70***	3,979.70
Air force planes	—	—	−31,125.00***	5,316.80
Navy planes	—	—	−38,963.90***	4,953.40
Navy missiles	—	—	−27,200.80***	4,387.30
Navy ships	—	—	−41,037.70***	5,383.70
R^2	.27	—	.67	—
F	12.42***	—	24.24***	—

Note: The amount concentration dummy is for air force missiles and army planes and vehicles; the unit concentration dummy is for army aircraft, missiles, and vehicles; all coefficients are multiplied by 10^5. N = 104, except for corrected amount concentration findings, where N = 65 with army programs excluded.

*$p \leq .05$.

**$p \leq .01$.

***$p \leq .001$.

in the service analysis by unit cost, it responded in the opposite way. (The unique army budgetary response can be attributed to army concentration in high-cost helicopter programs.) Still, evidence for a distributive effect of higher budgets is found in three of the four estimates, regardless of statistical method. (Neither the weapon budget coefficient nor the associated dummy are significant in the service–weapon analysis of amount concentration.)[25] In all, then, the analysis of the concentration equation reveals that on program concentration, the prior-year budget exerts a selective diversifying influence, the R&D–procurement balance exerts an expansive influence (controlling for budgeted amount in the service sample), and mission emphasis exerts a contractive *and* expansive influence (in the service–weapon sample).

CONCLUSION

The analysis shows that programmatic influences, expressed as program contraction and expansion, significantly constrained weapon budgeting in the Reagan–Bush era: a general expansion in service programs, including big budget areas (air force aircraft and navy ships), pushed up the weapon budget even as the budget also responded to the overall executive commitment to defense. While programs diversified, in turn, with budgetary opportunities and available technology, programming appears to have responded most to service missions (objectives): when turning to missions, the services increased program variety while concentrating program budgetary resources. The services thus moved into new areas while consolidating the service base.

Judging from budgetary outcomes, then, the services adopted an "active" stance and an "outward" orientation. The services thus conformed to the assertive image of the bureaucratic politics literature and appear to have adopted a programming strategy to claim future budgetary resources. Yet they were also mindful of the "business that they are in" and nurtured "essential" programs. And when increasing budgets for signature (service-defining) combat systems, the services tended to favor some systems over others.

Admittedly, the service personality is not fixed in stone. And the services are shown here acting under the exceptional conditions of the 1980s. In a time when money was plentiful and weapons seemed to provide their own rationale, the services, immune from criticism, were more than willing to promote their wares to secure a market. The services might act differently in times of austerity.

The analysis, then, establishes a continuing need to account for bottom-up budgetary influences. Studies are required in order to close the gap between nuanced, qualitative treatments of bureaucratic behavior and quantitative efforts to model these effects. These studies admittedly pose statistical challenges—not the least of which is to obtain useful data. But this behavior suggests no obvious alternative.

For in the end, defense budgets are neither simply creatures of strategic necessity nor bloodless responses to previous allocation decisions. They are contested and manipulated under conditions of information scarcity, uncertainty, and institutional deference that have long given the services bargaining advantages. Defense budgets are thus more intractable than suggested by either strategic or incremental perspectives. These budgets finally emerge as packages of nonchoices, with viable options hidden if not intentionally foreclosed.

In this post–Cold War transition period, the pressing question is whether the defense budget can be managed. At a minimum, a budget controlled from below creates inefficiencies, redundancies, and shortfalls. Beyond this, when open to decentralized influences, budgeting institutionalizes conflict. Self-interested bureaucracies maintain their strategies, tactics, programs, and budgets, insulated from global change.

APPENDIX

The variables and indicators are shown in Table A-1.

TABLE A-1

Variables and Indicators

VARIABLE	MEASURE	COMMENT
Concentration ACONC [amount] UCONC [unit]	$\sqrt{\Sigma(X_i/X)^2}$	X_i = individual program amount or unit costs; X = total program amounts or unit costs[a]
Weapon budget (WBDGT)	total weapon budget for specific planes, missiles, ships, and/or vehicles	total amounts budgeted for all programs
Defense commitment (DEFCOM)	total budget for procurement, R&D, O&M, excluding weapons	for analysis by service and weapon type, the budget is multiplied by service–weapon type WBDGT/service WBDGT
O&M trade-off (OMTOFF)	$(OM - OP)/(OM + OP)^b$	for analysis by service and weapon type, the budget is multiplied by service–weapon type WBDGT/service WBDGT
Mission emphasis (MISEMP)	budget for strategic weapons and combat platforms/weapon budget	for analysis by service and weapon type, the budget is multiplied by service–weapon type WBDGT/service WBDGT
R&D balance (RDBLNC)	$((RD - OP)/(RD + OP))^{2b}$	for analysis by service and weapon type, the budget is multiplied by service–weapon type WBDGT/service; WBDGT higher values signify an "imbalance"

Note: All measures are calculated either by service (air force, navy, and army) or service and weapon type (air force aircraft and missiles; navy aircraft, missiles, and ships; and army aircraft, missiles, and combat vehicles).

[a] Unit costs = program amounts controlling for quantity procured.

[b] CM = operations-and-maintenance, OP = other procurement (here, any nonweapon procurement), RD = research-and-development

Notes

I wish to thank Lee Sigelman and Robert Stoker for their comments and suggestions.

1. The period boasted a postwar high ratio of procurement and R&D spending to total defense spending (United States, General Accounting Office 1992).

2. Barton Gellman, "The 'Silent Service' Breaks the Ice: Submariners Test Waters for New Role as Soviet Threat Dries Up." *Washington Post*, 19 April 1992, sec. A.

3. Requirements are influenced by numerous political actors wielding an effective program "veto." This point, and a discussion of the case of the army Bradley Fighting Vehicle, are found in McNaugher 1989, 125, 129–31.

4. Weapon versatility is a less solid basis for analysis, because all of the high-profile weapon programs of the 1980s and 1990s were multipurpose.

5. For recent discussions of the weaknesses of the bureaucratic politics perspective, see Bendor and Hammond 1992 and Welch 1992.

6. Furthermore, bureaucratic politics implicitly downplays uncertainty and risk by promoting a heroic image of the decision maker as one able to manage complexity and uncertainty without formal, rigorous, and ongoing study and able to steer acquisition without proper oversight, risk management, learning and adaptation, or tailoring risk to actual need.

7. One attempt to answer these questions can be found in Clark 1984.

8. Because the service political strategy is future-oriented, program concentration and weapon budgets are assumed to operate through lagged effects, which do not require simultaneous equation estimates. The interdependent error terms found in simultaneous relationships complicate problems of obtaining estimates with a time-series cross-sectional design. Arguably, one problem cannot be adequately solved until the other is addressed.

9. If bureaucratic politics prevails, mission emphasis can change with budgeted amounts (and might not be strictly

endogenous in the model) but is always prior to programmatic concentration decisions.

10. I recognize that capability or opportunity can instill desire and influence objectives.

11. These data are for administration requests and are contained within the National Defense Authorization Act Reports (separately issued by the Senate Committee on Armed Services, House Committee on Armed Services, and Conference Committee) on authorizing military appropriations for the next fiscal year.

12. Program concentration measures were also based on administration budget requests rather than earlier service requests with the assumption that the services dominate programming decisions and that service preferences change during the lengthy programming and budgeting process.

13. Because annual outlays differ from annual authorizations and appropriations (money can be spent this year, though authorized and appropriated in a prior year), budgeted amounts were further distinguished. Budget concentration was calculated by summing (current and future) amounts authorized in a given year while unit concentration was calculated by summing (previous and current) amounts to be spent in that year (since the latter measures actual unit costs in a given year).

14. For unit concentration, a single weapon is assumed procured when no amount is designated, since these allocations are often associated with production start-up costs.

15. The weapon budget excludes the production costs and/or numbers for several classified programs. Most significant among them are the air force advanced cruise missile program and stealth attack fighter (the F-117) in the late and early 1980s, respectively. This study partly deals with this problem by testing the sensitivity of findings to the period examined. (In any case, the inclusion of these programs is likely to further enhance any relationship between program diversification and weapon budgets.)

16. Specifically designated marine procurement is omitted from the calculations, though some systems are used by the navy for marine support, and navy R&D budget includes marine systems.

17. "Other" procurement is used, in part, to avoid biased statistics when indicators calculated from weapon procurement are correlated with weapon procurement.

18. Budgets were corrected for inflation with the producer price index, with inflation rates assumed constant for fiscal year 1991–93.

19. Estimates are based on the assumption that budgets are divided among service programs in proportion to the ratio of service-weapon budgets to each service's overall weapon budget. In administration requests, these budgets are not broken down programatically, as they are in the weapon budget. (While "other" procurement is distinguished by weapon type in the annual budget, it has been treated differently over the years and now largely centers in a formally designated "other" procurement budget and, for the army, also an "ammunition" budget.)

20. 1980 data are introduced as a lag term.

21. To generate parsimonious estimators, dummies were first fitted for cross sections falling below predicted values and then to those falling above these values, if necessary.

22. The relationship between DEFCOM and the weapon budget is arguably made artificially high by the estimation procedure for the nonweapon budgets in the service–weapon sample, where these budgets are multiplied by the ratio of the service–weapon budget over the total service budget for weapons. Nevertheless, the same relationship is found in the service analysis (where estimates are not employed); and, if anything, the estimation procedure imposes a harsher test of significance on other model variables.

23. While the service–weapon estimates for the weapon budget equation (using unit-cost concentration) were obtained by first incorporating dummy intercepts to correct for negative residual groupings, the later correction for positive groupings yielded numerous nonsignificant dummy intercepts and a nonparsimonious equation. The nonsignificant terms were dropped and the equation reestimated. Interestingly, parameter significance (at the .05 level) was uneffected by the dropping of the intercept terms, and the residuals obtained spread evenly around predicted values.

24. Army programs complicated the analysis, with residual variance requiring dummy parameters and intercepts that severely increased the size of the model and washed out significant parameters. They were thus removed from the analysis to produce the findings reported here—findings consistent with the full-sample result, corrected for autocorrelation alone.

25. Given the problems of estimating the concentration equation (when applied to program amounts in the service–weapon sample) and the sensitivity of the findings to heteroscedasticity and service programs analyzed, equation estimates were derived separately for the 1981–86 period (a time of budgetary growth) and the 1987–93 period (a time of budgetary decline). In both periods, mission emphasis is again shown a highly significant source of contraction. Furthermore, the dummy variable—for air force missiles and army aircraft and vehicles—is linked with program expansion in the second period. Similar estimates for equation 1 generally confirmed the previous results, further establishing the robustness of those findings.

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REPRESSION OF HUMAN RIGHTS TO PERSONAL INTEGRITY IN THE 1980S: A GLOBAL ANALYSIS

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This crossnational study seeks to explain variations in governmental repression of human rights to personal integrity (state terrorism) in a 153-country sample during the eighties. We outline theoretical perspectives on this topic and subject them to empirical tests using a technique appropriate for our pooled cross-sectional time-series design, namely, ordinary least squares with robust standard errors and a lagged dependent variable. We find democracy and participation in civil or international war to have substantively important and statistically significant effects on repression. The effects of economic development and population size are more modest. The hypothesis linking leftist regime types to abuse of personal integrity rights receives some support. We find no reliable evidence that population growth, British cultural influence, military control, or economic growth affect levels of repression. We conclude by considering the implications of our findings for scholars and practitioners concerned with the prevention of personal integrity abuse.

In the past two decades there has been a burgeoning of information on governmental terror and the abuse of internationally recognized human rights in countries around the globe (see Cain, Claude, and Jabine 1992).[1] While the development of theories to explain why, and to predict when, such crimes will be committed would seem to be a vital undertaking, social science scholars have only begun to use the newly developed information toward this end. To date only a few studies have attempted to construct and test theories seeking to explain variations in the levels of repression found in countries around the world (Henderson 1991, 1993; McKinlay and Cohan 1975, Mitchell and McCormick 1988; Park 1987).

We seek to build upon the strengths of existing empirical studies on this question, while improving upon them in several respects. We therefore construct a model of the most dramatic form of repression—repression of personal integrity rights—that tests several hypotheses suggested by previous studies, but goes on to test hypotheses suggested by theories not considered in previous empirical work on repression. The data set we employ clearly represents the most comprehensive yet analyzed in a global study of national human rights practices in terms of the number of countries and years it covers, as it includes relevant characteristics and behaviors for a pooled cross-sectional time-series sample of 153 countries for the eight years 1980–87.

RESEARCH ON HUMAN RIGHTS AND REPRESSION

Recent years have seen the publication of several empirical studies relating to human rights, but the tendency has been to focus on human rights practices as an independent variable, specifically as a determinant of foreign policy outputs.[2] The first studies that sought to explain variations in human-rights-related phenomena apparently were conducted by McKinlay and Cohan (1975, 1976), who analyzed the general policy performance of military, as compared to civilian, regimes. For the period 1951–70, McKinlay and Cohan (1975) compare the performance of their two types of regimes with respect to a number of "political variables" relevant to human rights—the proportion of regime-years in which constitutions were declared not in full force and the proportions in which assemblies, political parties in general, or the Communist party were banned. Their statistical analyses demonstrated that poor regimes were more likely than richer ones—and military regimes more likely than civilian ones—to ban constitutions, assemblies, and parties.

McKinlay and Cohan's work was accomplished before Amnesty International, the U.S. Department of State, or Freedom House had begun to publish their assessments of human rights performance annually and with worldwide coverage. More recently, spurred by increasing public and scholarly interest in human rights issues, a few studies have appeared that seek to explain the variations in these more general assessments of regime performance in human rights. In a brief article, Park (1987) reports statistically significant positive relationships between political rights, evidently measured by inverting the Freedom House civil political rights index, and a physical quality of life index, urbanism, welfare expenditures, ethnic diversity, and percent Christian population. He reports statistically significant negative relationships for political rights with military expenditures, education expenditures, and percent Muslim population and no relationship for inequality and political rights.

Ambiguities in Park's discussion of measurement procedures and the bivariate and essentially atheoretical nature of his statistical analyses make us wary of the relationships that he found. Nevertheless, Park's research is potentially useful as a source of implied hypotheses to be tested in a more rigorous framework.

More recently Mitchell and McCormick (1988) went well beyond Park's study by framing several hypotheses to explain national violations of the integrity of the person through the use of imprisonment and of torture, assessed through their ratings of the reports issued by Amnesty International in 1984. Their analyses indicated that economic conditions, as measured by per capita gross national product (GNP), were negatively, if moderately, related to both types of repression and that experience with British colonial rule and authoritarian (as opposed to totalitarian) form of government, were significantly negatively related to the taking of political prisoners but not to the use of torture. Capitalist trade and investment (dependency) and length of independence, on the other hand, were not significantly related to either aspect of repression.

The strength of Mitchell and McCormick's analysis lies in its delineation of hypotheses that theoretically might explain, in part, variations in repression. Its major weakness, as the authors acknowledge, is that its analyses of these theoretically important relationships are effectively bivariate.

The most recent additions to the literature seeking to explain variations in repression are a pair of studies by Henderson (1991, 1993). Henderson hypothesized that democracy, economic growth, and economic development would reduce—and "socioeconomic needs" and inequality would increase—levels of repression (1991, 123–27). His multiple regression model for repressive behaviors, using a measure created from the State Department's *Country Reports on Human Rights Practices for 1985*, confirmed his hypotheses concerning the effects of democracy (negative), inequality (positive), and economic growth (negative) on repression but found no significant relationship for level of economic development.

Henderson (1993) focuses on the impact of population variables on his governmental repression measure for 1985. Henderson theorizes that two aspects of population—density and rate of growth—have an effect on propensities to repress. He reports finding "a meaningful relationship" between population growth and political repression but fails to turn up much evidence of a similar relationship with population density.

Though suggestive, the research we have just summarized has some limits that impede our ability to formulate an understanding of the societal determinants of respect for personal integrity and the practice of state terror. With one exception, the studies are cross-national, cross-sectional sample designs that take no account of the change that can occur within a given country.[3] With two exceptions (Henderson 1991, 1993), the studies use little more than bivariate statistical methods to test hypotheses that require multivariate formulation. Also, only one study uses a more or less comprehensive sample of countries (Henderson 1993).

A final difficulty for those who wish to draw inferences from this literature is that the analysts employ four different dependent variables to measure respect for human rights or governmental coercion: formal measures of the effectiveness of representative institutions (McKinlay and Cohan 1975, 1976), a Freedom House rating (Park 1987), a rating derived from an Amnesty International report (Mitchell and McCormick 1988), and a rating derived from a State Department report (Henderson 1991, 1993).

We shall construct a model to test hypotheses suggested by contending theories of *state terrorism* and *human rights abuse*. A first necessary step toward that end is to define what we mean by these terms and to operationalize them using available measurement techniques.

DEFINING AND MEASURING HUMAN RIGHTS AND STATE TERRORISM

The problem of measuring human-rights-related concepts has received much attention in recent years. The problem of finding a consensual measure of any such concepts is likely an impossible one, due to inevitable disagreements regarding values and definitions (e.g., Van Dyke 1973). We believe, however, that researchers should carefully define the terms and describe the operationalizations adopted, in order to minimize confusion and enhance replicability.

We shall focus on the subset of human rights categorized as dealing with the "integrity of the person" (see Cingranelli and Pasquarello 1985; Henderson 1991, 1993; Mitchell and McCormick 1988; Stohl and Carleton 1985). We also refer to violations of these rights as instances of *state terrorism*, which, consistent with Gurr (1986), we consider to be a category of coercive activities on the part of the government designed to induce compliance in others. Examples of such activities include murder, torture, forced disappearance, and imprisonment of persons for their political views.

Our adoption of these definitions is not meant to suggest that components of broader definitions of human rights—including rights economic, political, or social in nature—are unfounded or that they are unimportant. We simply focus our current efforts on the integrity of the person because we believe governments abusing this right are committing the most egregious and severe crimes against humanity and that these violations are of the sort that can usually be avoided.[4] Further, limiting the term to this category of rights allows us to separate the concept of human rights from related concepts (e.g., democracy, economic standing) that may be, or have been, linked theoretically with national propensities to respect human rights (e.g., Henderson 1991, 1993; Mitchell and McCormick 1988).

Even after we specify the subset of human rights with which we are dealing, difficulties with measurement of the concept remain to be solved. The evolving consensus among researchers is that the development of better data sources would be helpful but that sufficient data have been developed to begin the theory building and falsification enterprise, through the use of empirical tests.[5]

We have chosen to adopt the standards-based approach, as opposed to the events data approach described by Stohl and his colleagues (Lopez and Stohl 1992; Stohl et al. 1986).[6] Fortunately, relatively recent developments in the collection of data on human rights offer us measurement alternatives that fit our purposes well. Two groups of researchers have applied similar sets of standards, each placing countries on five-point ordinal human rights scales, according to their reading of human rights information sources. Mitchell and McCormick (1988) created different variables to represent what they considered to be the two conceptually distinct dimensions of torture/killing and imprisonment, according to their reading of the Amnesty International Reports. Stohl and his colleagues (n.d.) have opted to include torture, imprisonment, and political killings and executions on the same ordinal scale and to create ratings by analyzing the reports of both Amnesty International and the U.S. Department of State.

While either classification would be acceptable, we have chosen to use the classificatory system employed by Stohl and his colleagues (Carleton and Stohl 1987; Stohl and Carleton 1985; Stohl et al. n.d.). We believe that it can be persuasively argued that the two dimensions postulated by Mitchell and McCormick stem, in reality, from the one dimension that Stohl and his colleagues tap—that both torture/killing and imprisonment are rooted in a regime's willingness to repress its citizens when they are considered a threat. To operationalize repression for this study, we used standards-based data (Poe and Sirirangsi 1993, 1994), as well as data generously provided by Stohl and his colleagues that appears in their unpublished work (Stohl et al. n.d.) and have been used in their previous studies (e.g., Carleton and Stohl 1987; Gibney and Stohl 1988; Stohl and Carleton 1985; Stohl, Carleton, and Johnson 1984). Following them, our data includes two five-point ordinal human rights indices generated through analyses of the contents of both the State Department and Amnesty International reports.[7] These five-point scales of human rights abuse range from 1 for a country with a healthy record of respect for personal integrity (Canada, the United States) to 5 for a human rights disaster (Iran during several years in the 1980s). The scale and further information regarding the coding of cases are presented in the Appendix.

In coding the cases from the profiles available in the Amnesty International and State Department reports, it became evident that the samples of countries covered by those two sources were somewhat different. Amnesty International was not as comprehensive in its coverage as the State Department: Amnesty provided an average of 132 national profiles per year with sufficient information to code, while the State Department covered an average of 151. The simplest way to deal with this problem would be to conduct analyses on the cases that are available for each measure. But there is good evidence that this would bias the findings, since Amnesty tends not to produce profiles for countries that have respectable human rights records.[8] Because of the rather high correlation between the two measures in our sample (zero-order correlation = .83), we instead chose to substitute the value coded for the State Department scale when profile information was unavailable on a country in the Amnesty International reports and vice versa (in the few cases where it was necessary) as the best available approximation of those scores.

Following the practice in several of the Stohl studies, parallel analyses are conducted with the two indicators. This provides us with a check against nonvalid findings that might arise due to biases in either indicator, an advantage not found in previous studies explaining variations in human rights behavior. Where the results gained with the two indicators are similar, we can be more confident that our findings are not due to biases in the measures but are, in fact, "real".[9]

BUILDING A MODEL OF HUMAN RIGHTS ABUSE

Now that we have defined and operationalized the dependent phenomenon, we shall outline the theoretical perspectives on repression from which we draw the hypotheses that we shall test. Having outlined the theoretical justification for testing an hypothesis, we then specify how each of the key concepts is operationalized in our model.

Democracy

That democracy ought to decrease governmental resort to terrorism is strongly argued by Henderson: "The democratic process, with its emphasis on bargaining and compromise, offers a meaningful alternative for handling conflict if leaders choose to use it. Democracy should not be viewed as an idealistic process, but as a realistic way to accommodate demands with a minimum of conflict. . . .With a large measure of democracy, conflict should not grow so sharp as to invite repression[11] (1991, 123–24). Henderson goes on to note that democracy "cannot be based on pseudoparticipation. There must be legitimate channels, such as political parties and elections, that can carry interests forcefully into government" (p. 124).[10]

The dampening effect of democracy on conflict surely does not represent the only way in which it inhibits repression. Effective democracy also provides citizens (at least those with political resources) the tools to oust potentially abusive leaders from office before they are able to become a serious threat. In addition, the freedoms that are essential to procedural democracy may make it easier for citizens and opposition leaders to publicize attempts at repression, thereby bringing down on would-be abusive leaders the weight of majority or world opinion.

While the arguments relating democracy to human rights are strong, there can be a problem of tautology when one tries to put democracy and human rights abuse into an independent–dependent variable relationship. Certain minority and procedural rights are normally regarded as elemental features of a democratic form of government. If the rights that must be respected in a democracy are defined very broadly, they may well merge imperceptibly into the respect for human dignity that is, by definition, antithetical to the use of state terrorism.[11] If democracy is to function as an independent explanation for state terrorism and abuse of personal integrity, it must be defined in terms of procedures and rights that do not themselves preclude repression, even though they may represent considerable respect for human rights other than those most closely related to the integrity of the person. In addition, if we are empirically to examine the relationship between democracy and repression, democracy must be defined in terms that allow independent operationalization of the concept for the sample of nations we analyze.

For a theoretical definition of democracy, we turn to the work of Bollen, who, after surveying the definitions of a number of leading democratic theorists, defines *political democracy* as "the extent to which the political power of the elite is minimized and that of the nonelite is maximized" (1980, 372). Since *power* is no more easily operationalized than *democracy*, Bollen follows the lead of other theorists in noting the crucial role of political liberties and in seeing elections as mechanisms that "may increase the power of the nonelite." If they are to increase the power of the nonelite, elections must be fair, allow choice, be based on a universal franchise, and have results that are "binding on all parties": "Political liberties refer to the rights of all individuals and groups to protest or support—freely—government policies and decisions." These "provide additional political power for the nonelite, allowing them to organize opposition to the elites and their policies" and include free speech, free press, and freedom of opposition (p. 372).[12]

Two measurement alternatives suggest themselves as useful indicators for the concept of democracy as it has been defined. These are the measure of democracy used by Vanhanen (1990) in his studies of democratization and the Freedom House indicator of political freedom (see Gastil 1988; 1990). A third alternative, the index of "institutionalized democracy" in Gurr's Polity II data would have been an ideal candidate for use in our models (1990, 37–38); but it is not available for the year 1987, the final year covered by our data set.[13]

Vanhanen's index concentrates exclusively on the electoral components of democracy: it is the percentage of the population actually voting in a given election times the difference between the percentage of the votes won by the largest party and 100%. The index is zero for nations whose governments were not elected. Since it is multiplicative, it can also be zero or near zero for a nation that lacks party competition or has an extremely low turnout percentage. Though noninstitutional, a strength of Vanhanen's index is that, like the Gurr index, it does not depend upon the extent to which a nation respects the dignity of the person. And unlike the Polity II measure, Vanhanen's index is available for the whole period of our analysis and for 142 of our maximum of 153 countries with full data on our dependent and other independent variables. (Pooled N using the Vanhanen indicator = 1,136.) We were, however, somewhat uncomfortable with the exclusively electoral focus of Vanhanen's index.

Thus the second measure of democracy that we use is the Freedom House index of political rights.[14] Our examination of the criteria used to create the Freedom House political rights indicator convinced us that it can serve appropriately as an indicator of democracy, even if others have used *Freedom in the World* indicators as alternative measures of the state of human rights (Park 1987; Wesson 1987, pref.).[15] According to a recent volume of *Freedom in the World*, the Freedom House index of political rights assesses

> whether the people have "the right to vote and compete for public office";
> whether elected representatives "have a decisive vote on public policies";
> whether "the people have a choice in determining the nature of the system and of its leaders";
> whether chief executives and legislative representatives "are elected through free and fair elections";
> whether "there are fair electoral laws, equal campaigning opportunities, fair polling and honest tabulation of ballots";
> whether "voters are able to endow their leaders with real power, or whether unelected elements reduce or supersede this power";
> whether the system allows "the people to organize in different political parties or other competitive political groupings of their choice" and is "open to the rise and fall of these groups";
> whether citizens are free "from domination by the military, foreign powers, totalitarian parties, religious hierarchies, economic oligarchies or any other powerful group"; and
> whether "cultural, ethnic, religious and other minority groups have reasonable self-determination, self-government, autonomy or participation through informal consensus in the decision-making process" (McColm 1990, 19–20).[16]

This measure is by no means ideal for our purposes. The political liberties ratings supplied by Freedom House do not include specific assessments of the freedoms of speech and press that are included in Bollen's index of democracy.[17] The indication that the political rights measure includes "freedom from domination" by unelected groups also raises the possibility that it might slightly overlap with our measures of abuse of personal integrity.[18] Finally, the Freedom House indicators have been criticized as "highly impressionistic, being no more than an estimate by a person who has collected a lot of seemingly relevant information on all the countries of the world" (McCamant 1981, 132.)

Despite these criticisms, we use the Freedom House political rights index as one of our two primary measures of democracy in the analyses that follow, for several reasons. First, precision in the statement of the Freedom House measurement criteria has improved over the years so that criticisms based on measurements for the 1970s are less persuasive for the 1980s. Second, the Freedom House indicators have been used successfully in a number of studies of freedom-related concepts (see Poe 1991, 1992; Rummell 1983; Stohl, Carleton, and Johnson 1984). Third, the Freedom House indicator of political rights is available for the largest number of countries (153) for which we have human rights measures for the years 1980–87, thus allowing us to conduct the most extensive possible analysis. Fourth, recent research by Bollen has found the Freedom House political rights indicator to have the highest validity rating among the several he analyzed, suggesting that if one is to use a single indicator, the political rights measure is certainly a good one (1993, 1225).[19] Finally, and most important, our extensive statistical analyses of the three available measures of democracy indicate that they are so highly correlated as to be virtually substitutable and that there are few substantively interesting differences in the pooled cross-sectional time-series models of repression one might compute using the three indices of democracy. The seven-point ordinal scale is inverted in our analyses, so that more democratic countries achieve larger scores.

In sum, we believe the Freedom House indicator is a useful measure of democracy. Since we are employing this measure with the Vanhanen index, in parallel analyses, it is unlikely that any of its weaknesses will lead us to invalid findings. Democracy as we have now defined it is, in concept, substantially independent of human rights abuse. Furthermore, the results of preliminary analyses indicate that the two concepts are relatively independent empirically.[20]

Population Size and Growth

Henderson develops a detailed and persuasive argument that population size and growth increase regime tendencies to use repression: "Growth in numbers of people can create scarcity—a short-fall between what people need and want and what they have. Under this pressure governments may be pushed in an authoritarian direction. . . . What is worse, government may resort to repression as a coping mechanism" (1993, 8).

A large population may increase the occurrence of state terrorism in at least two ways. First, a large number of people increases the number of occasions on which such coercive acts can occur. As a matter of simple probability, such an increase should lead to the occurrence of more instances of coercion. Second, a large population places stress on national resources and bring the threat of environmental deterioration, further reducing available resources (ibid).

Arguments made by Henderson indicate that rapid population growth may also promote resource stress, perhaps even more than population size: "The extent of scarcity varies from country to country, but in the more hard-pressed countries, burgeoning demands will keep governments off-balance and will incline them to resort to repression. Growing populations absorb any economic growth rate that may occur, thus frustrating governments' efforts" (1993, 4). Rapid population growth also increases the proportion of the population falling into the youngest age categories that require the constant creation of new jobs, new housing, and many other government services, as well as posing the greatest tendency to engage in criminal activities and other threats to public order (ibid. and the sources cited therein).

To operationalize the effects of population on the abuse of personal integrity through state terrorism, we employ two variables: the natural logarithm[21] of total national population and the average percent increase in national population from one year to the next, over the eight-year period of the study.[22]

Level of Development and Economic Growth

Despite the use of different indicators for both independent and dependent variables, the research we have summarized uniformly found a negative relationship between level of economic development or wealth and repression (Henderson 1991; McKinlay and Cohan 1975, 1976; Mitchell and McCormick 1988; Park 1987). The logic underlying these results appears straightforward: "The poorest countries, with substantial social and political tensions created by economic scarcity, could be most unstable and thus most apt to use repression in order to maintain control" (Mitchell and McCormick 1988, 478). Again, "It is only logical to think that, with a higher level of development, people will be more satisfied and, hence, less repression will be needed by the elites" (Henderson 1991, 1226). The only caveat to these musings comes from Mitchell and McCormick's interpretation of Huntington's argument concerning political instability suggesting that the use of coercive measures might rather be expected to be most serious in states in the middle, transitional levels of development, an argument for which they find little empirical evidence (Mitchell and McCormick 1988, 479, 488–90).

The presumed effects of economic growth are more problematic. On the one hand, it is logical to assume that rapid economic growth, since it expands the resource base, should reduce the economic and social stresses that lead governments to use terrorism as a policy tool. But there has also been a strong argument that rapid economic growth is most likely to be a destabilizing force that will, in fact, increase instability and a regime's temptation to resort to coercive means to maintain control. (Olson 1963 is the seminal work.) The destabilizing effect of rapid growth may occur because it can rarely be rapid enough to outstrip the growth in expectations that is simultaneously occurring (Gurr 1970, 1986); because it increases the number of declassé individuals and groups most prone to promote instability (Olson 1963); or because rapid growth inevitably occurs unevenly, possibly even creating growth by decreasing the well-being of the nonelite. In any case, frequently sharpening class differences within the population cause the elite to promote repression to keep the nonelite quiet (Henderson 1991, 126 and the sources cited therein).

We thus expect abuse of the right to personal integrity to be decreased by level of economic development but increased by economic growth. When we turn to operationalizing economic development and growth, we find some controversy over how these variables ought to be measured. Dissatisfaction with the defects of the most traditional of indicators—gross national product (GNP) per capita—has led to the development of several alternative measures of economic standing.[23] The major problem with these alternatives is that they do not exist—and cannot easily be created—for most of the years, or for a significant number of the countries, of our study. As a result we follow Mitchell and McCormick (1988) and McKinlay and Cohan (1975, 1976) in using GNP per capita and percentage growth in GNP per capita as our measures of economic development and economic growth. Despite the deficiencies in these measures, we have little reason to believe they produce inaccurate or misleading results compared to those achievable with other currently unavailable alternatives.

Leftist Regimes

The argument of former U.S. ambassador to the United Nations Jeane Kirkpatrick (1979) that "totalitarian" (i.e., noncompetitive, Marxist or Marxist–Leninist) regimes were the world's most repressive is reviewed and analyzed by Mitchell and McCormick (1988, 480–81, 493–95). They find that such regimes are, in fact, more repressive on at least one of their dimensions of repression than are "authoritarian" regimes of a non-Marxist nature. Such a finding is not surprising if one takes seriously the tenets of Marxist–Leninist theory about the need for a dictatorship of the proletariat, and some other communist doctrine.

Even so, critics of U.S. foreign policy have taken Kirkpatrick and the State Department to task for what the critics see as their efforts unfairly to paint all socialist regimes with the tar of repression. We test the hypothesis that leftist regimes will be more coercive than other regimes, where leftist regimes are defined quite precisely as those governed by a socialist party or coalition that does not allow effective electoral competition with nonsocialist opposition.

Military Regimes

That we expect military regimes to be more coercive than others probably surprises no one, whether they are familiar with McKinlay and Cohan's findings or not. Military juntas are based on force, and force is the key to coercion. Yet, in many of the nations in which soldiers forcibly take power, they do so alleging that the leaders they are replacing were themselves violating the constitution and, possibly, engaging in repression of the rights of the citizens. In addition, since military rule is by definition antithetical to democracy, it might be that any apparent relationship between military rule and state terrorism is spurious, a result of a failure to control for the democratic/nondemocratic nature of the regime.

For our operationalization of military-controlled regimes, we use a classification created under our direction by Madani (1992) from the *Europa Yearbook.* Following McKinlay and Cohan, the classification defined military regimes as those which had come to power "as a consequence of a successful coup d'etat, led by the army, navy or air force, that remained in power with a military person as the chief executive, for at least six months in a given year" (p. 61; see McKinlay and Cohen 1975, 1). The category *military regimes* also included a small number of mixed regimes "with either a civilian as the chief executive and several military persons in the cabinet or a military head of government who nominated a civilian as the head of government and himself worked behind the scenes" (Madani 1992, 61). All other governments were considered civilian regimes.[24]

British Cultural Influence

Mitchell and McCormick argue that the political cultures of nations may have an important impact on the extent to which their governments engage in repression and that "one important factor that is thought to have shaped political culture for most states is the colonial experience" (1988, 479–80). Specifically, they note: "British colonial rule . . . is commonly thought to be strongly associated with the postcolonial development of democracy. The British legacy may be a relatively greater respect for human rights. By contrast, other colonial experiences (Spanish, for instance) are generally assume to have introduced a greater degree of hierarchy and authoritarianism. The legacy here may well involve higher levels of human rights violations" (p. 480).[25]

This is certainly not the place to review the efficacy of political cultural explanations of national political behavior. At their best, they connote that certain attitudes inculcated by the culture, but not directly measured, are partially responsible for differences in the dependent behaviors of interest. That is the direct implication of Mitchell and McCormick's argument, and, following their lead, we include "British cultural influence" in our models of state terrorism. British influence is therefore represented in our model by a variable coded 1 for countries that had been territories of Great Britain at some point during their histories, which all other countries coded 0.

International War Experience

In their study of economic summitry, Putnam and Bayne (1987) draw on game theory to illustrate that when regime leaders ascend to power, they face the prospect of playing simultaneously in two distinct but nevertheless interrelated games: one is played in the domestic political arena, with the primary purpose is to keep power, whether through election or the use of terror; the other takes place in the international realm, with other major players being the leaders of other countries, their representatives, and relevant international governmental and nongovernmental organizations. We believe that this analogy is also useful to researchers interested in explaining human rights abuse. The fact that these two games are intimately intertwined suggests that leaders' actions in the domestic political realm will likely be affected when their nations are a direct participant in an international crisis situation. In fact, studies of what Stohl (1980) has called the "nexus of civil and international conflict" abound, and the results of some systematic empirical studies (e.g., Stohl 1975, 1976) tend to point to the conclusion that there is a positive linkage between participation in international war and the levels of domestic political violence in participant countries. In the only study we know that deals directly with the question of whether participation in international war affects political repression, Rasler (1986) focused on the twentieth-century United States and found evidence that administrations did indeed increase levels of repression during wars.

We hypothesize that such a relationship applies generally in our worldwide sample. As a guide to our coding of the concept *war* during the 1980s we used as a guideline the criteria developed by Small and Singer (1982). These researchers coded a country as having been a participant in an interstate war when (1) there was a total of a thousand or more battle deaths suffered by all of the participants in the conflict, (2) the particular country suffered at least a hundred fatalities or had a thousand or more personnel taking part in the hostilities (pp. 50, 55). We ignored a further criterion employed by the Small and Singer study—that a participant country should be a member of the "international system" (p. 51)—because it did not fit well with the purposes of this cross-national study, which includes countries regardless of whether they are members of the international system as defined by Small and Singer.

Civil War Experience

Just as governments may employ repression when threatened on the international front, it is also a tool commonly used by governments that are faced with internal problems (see Nieburg 1969; Skocpol 1979; Tilly 1978). The most serious of threats in the domestic arena is posed by a condition of civil war, in which the authority of the regime in power is being challenged by an armed and organized resistance controlled by a shadow government. Therefore we shall propose a hypothesis that to our surprise had not yet been tested in quantitative studies of human rights and state terrorism—that regimes are more coercive when they are involved in civil conflict.[26]

In order to operationalize the concept of civil war we again look to the criteria developed in conjunction with the Small and Singer (1982) study for guidance. First, the government, as the central authority in a country, must be involved as a direct participant in the war. Second, there must be an effective resistance, that is, either both sides must be "organized for violent conflict" or "the weaker side, although initially unprepared [must be] able to inflict upon the stronger opponents at least five percent of the number of fatalities it sustains" (p. 215).[27] Thus genocides and massacres are not considered to be civil wars, and this concept is kept distinct from our dependent variable.

TESTING A MODEL OF ABUSE OF PERSONAL INTEGRITY RIGHTS

In recent years studies employing the pooled cross-sectional time-series (PCT) design have appeared much more frequently in political science journals. The PCT design is especially appealing because it enables researchers to test theories over both space and time simultaneously and thus to witness the interplay of two dimensions usually viewed separately in either cross-sectional or time-series studies (Clarke 1992; Sayrs 1989; Stimson 1985). With this notable advantage, however, comes some statistical difficulties. Specifically, autocorrelation and heteroscedasticity often complicate efforts to apply ordinary least squares (OLS) regression, probably the discipline's most widely used and understood statistical technique, on PCT data. Both autocorrelation (also known as serial correlation) and heteroscedasticity may lead to inaccurate estimates of the standard errors of parameter estimates, thus calling into question the results of significance tests (Ostrom 1990; Stimson 1985).

Many PCT studies have used what Beck and Katz (1994) call the feasible generalized least squares method to deal with these problems. This is essentially generalized least squares (GLS) as it is usually practiced, where an estimate of the error process is generated to evade or overlook the assumption underlying GLS, that the error process is known. But Monte Carlo trials undertaken by Beck and Katz show that there is good reason to be concerned about violating this assumption, as FGLS is shown consistently to underestimate standard errors (1994, 15). As a result this method sometimes yields extremely optimistic estimates of statistical significance. We are therefore persuaded by the recommendations of Beck and his associates (Beck and Katz 1994; Beck et al. 1993) who favor the use of OLS regression, in tandem with a variation on White's (1980) robust-standard-errors technique that Beck and Katz developed for use on panel data, to control for heteroscedasticity.[28] The White technique provides a consistent or "robust" estimate of the standard error by estimating a parameter covariance matrix that is consistent in the presence of heteroscedasticity. In order to control the effects of autocorrelation that we found to be a problem in our preliminary analyses, we shall employ a lagged dependent variable in the model, along with the substantive variables already outlined.[29]

To review, our general model is as follows:

$$\begin{aligned}\text{Personal Integrity Abuse}_{tj} &= a\\ &+ B_1\,\text{Personal Integrity Abuse}_{(t-1)j} + \beta_2\text{Democracy}_{tj}\\ &+ \beta_3\text{Population Size}_{tj} + \beta_4\text{Population Change}_{tj}\\ &+ \beta_5\text{Economic Standing}_{tj} + \beta_6\text{Economic Growth}_{tj}\\ &+ \beta_7\text{Leftist Government}_{tj} + \beta_8\text{Military Control}_{tj}\\ &+ \beta_9\text{British Cultural Influence}_{tj}\\ &+ \beta_{10}\text{International War}_{tj} + \beta_{11}\text{Civil War}_{tj} + e_{tj}.\end{aligned}$$

Data generated from Amnesty International and U.S. State Department human rights profiles will be used to construct two different measures of the level of personal integrity abuse, and parallel analyses will be conducted on these measures. Further, we shall conduct tests with the Vanhanen and the Freedom House political rights measures of democracy with both of these dependent variables, so that in all, four models will be tested. Though we gathered data for the 1980–87 period, the addition of the lagged dependent variable meant that we had to discard 1980 from our final analyses.[30]

Modeling Personal Integrity Ratings Derived from Amnesty International Reports

Our first set of analyses seeks to explain variations in the repression scale generated from the Amnesty International reports, using the statistical techniques described. Under model 1 and model 2 of Table 1 we present the coefficients and standard errors obtained in these tests. The statistics for model 1 are those yielded when the Freedom House political rights rating was used to operationalize the concept of democracy. Those for model 2 are the results when we substituted the Vanhanen democracy measure for that of Freedom House.

The strongest predictors of the repression scores operationalized from the Amnesty International reports are, as might be expected, the lagged dependent variables that achieved large, statistically significant coefficients in both analyses. The inclusion of the lagged dependent variable on the right side of the equation represents our means of correcting for the serial correlation present in our time-series data. Nevertheless, the substantive meanings of the strong coefficients for the lagged endogenous variable are worth considering. Such coefficients mean that our individual country scores on state terrorism are strongly seated characteristics of their political systems that do not change easily or rapidly. In addition, these coefficients provide a mechanism through which the effects of changes in the independent variables at a given time (time_t) continue to influence the dependent variables beyond the time of the occurrence of the change. We shall use this characteristic to illustrate the dynamic effects of our independent variables on repression of personal integrity rights.

Once the effects of autocorrelation are controlled by incorporating these lagged variables,[31] several others are shown to be important determinants of levels of state terrorism. Our results indicate that level of democracy has a strong and statistically significant impact on governments' respect for personal integrity. Both the Freedom House and Vanhanen measures of democracy prove to be statistically significant determinants of national propensities to use repression. The smaller coefficient of the Vanhanen variable should not, however, be interpreted as being evidence that this variable has a smaller effect, since that variable is measured on a scale ranging from 0 to 46.4, as compared to the Freedom House variable which ranges from 1 to 7. In fact, when one considers the different scales of these two indicators, the coefficient of the Vanhanen scale is somewhat stronger than that of the Freedom House variable. We find that if a country were to achieve the least democratic score on the Vanhanen measure after having achieved the most democratic score in the previous time period, this would make a difference of about .4 in the dependent variable at time_1. While the initial impact is moderate, we can see after performing some simple calculations, considering the dynamic effect mediated by the lagged dependent variable, that the impact of a loss of democracy increases substantially over time.[32] Figure 1 shows that if the abandonment of the democratic process in our example country were to continue, other factors held equal, the human rights index would have increased by about 1.3 (on the five-point state repression scale) after five years, (i.e., at time_{t+5}) as a result of the lagged effect, stabilizing at 1.4 points greater than the original score a few lags thereafter. The movement from most democratic to least democratic on the Freedom House measure is associated with a .26 increase in the Amnesty index, at time_t, stabilizing at about a one-point rise in that index after several lags.

Relating these effects back to the scale of the dependent variable, if a democratic country with a near perfect human rights record were suddenly to abandon the democratic process, we would expect that the country would, at the very least, begin to hold some political prisoners, and that political brutality, executions, and murders might become a common feature of life, other relevant factors being equal. Is such a complete abandonment of democracy an

TABLE 1

Pooled Cross-sectional Tests of Explanations of Abuse of Personal Integrity Rights

	AMNESTY INTERNATIONAL		STATE DEPARTMENT	
	DEMOCRACY MEASURE:		DEMOCRACY MEASURE:	
INDEPENDENT VARIABLES	FREEDOM HOUSE (MODEL 1)	VANHANEN (MODEL 2)	FREEDOM HOUSE (MODEL 3)	VANHANEN (MODEL 4)
Constant	−.02	.07	.11	.23
	(.14)	(.12)	(.16)	(.18)
Personal integrity abuse$_{t-1}$	.73**	.73**	.63**	.64**
	(.08)	(.08)	(.10)	(.10)
Democracy	−.05**	−.009**	−.06**	−.01**
	(.02)	(.003)	(.02)	(.003)
Population size	.05**	.04**	.06**	.04**
	(.02)	(.02)	(.02)	(.02)
Population change	.008	−.004	.01	−.002
	(.009)	(.01)	(.02)	(.01)
Economic standing	−.008*	−.007	−.02**	−.01**
	(.004)	(.004)	(.004)	(.004)
Economic growth	−.0009	−.0005	.0003	.00007
	(.001)	(.001)	(.001)	(.001)
Leftist government	−.04	−.03	.08*	.11**
	(.07)	(.07)	(.04)	(.04)
Military control	.05	.05	.04	.06
	(.05)	(.06)	(.05)	(.06)
British cultural influence	−.03	−.05	−.01	−.04
	(.05)	(.06)	(.04)	(.04)
International war	.21**	.22**	.34**	.35**
	(.07)	(.07)	(.09)	(.01)
Civil war	.33**	.33**	.49**	.49**
	(.11)	(.11)	(.12)	(.12)
R^2	.77	.77	.75	.75
N	1071	994	1071	994
Average contemporaneous Correlation of Errors	.43	.43	.45	.43

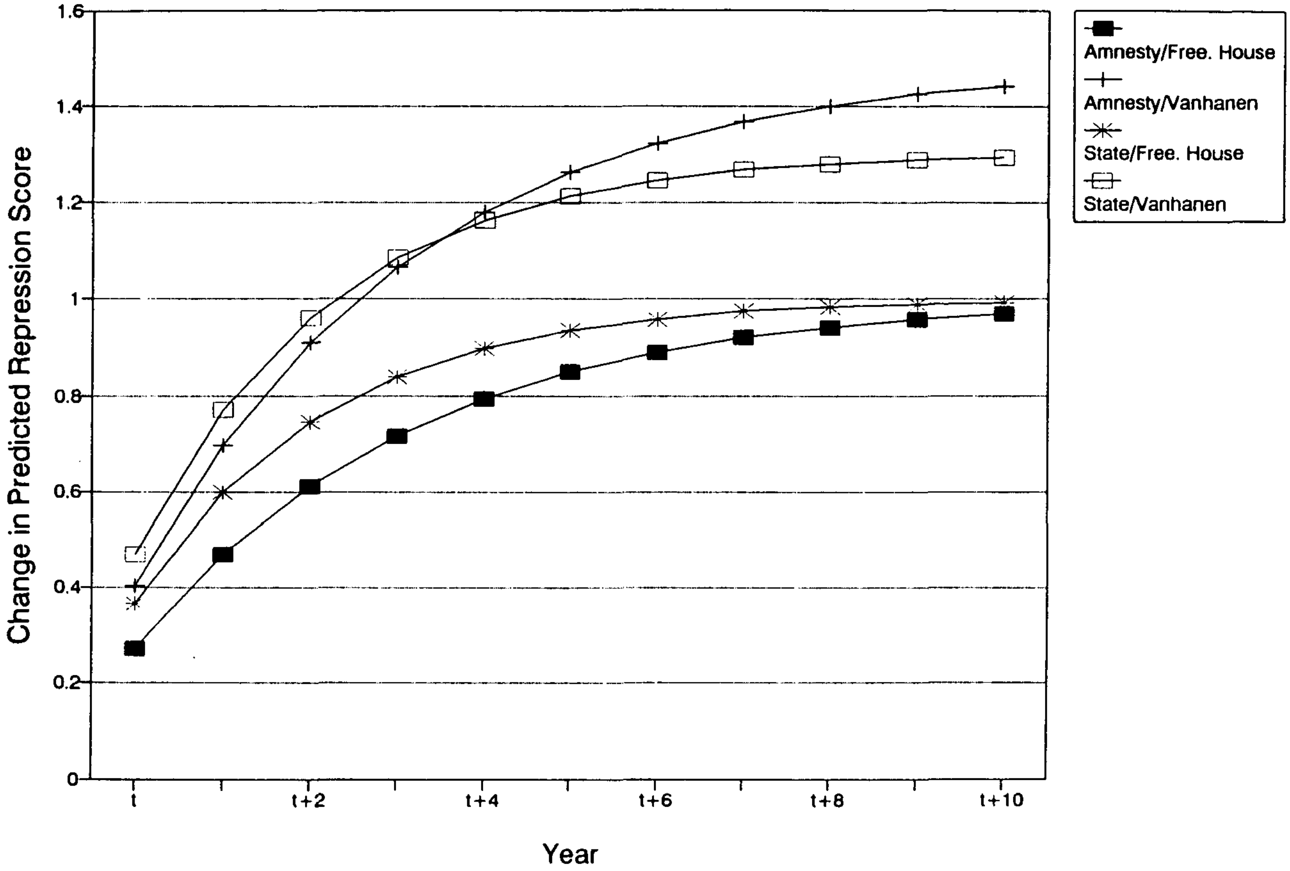
Note: Main entries are unstandardized OLS coefficients, generated using RATS 386 version 4.02. A lagged dependent variable is included in each model to control the effects of autocorrelation. The robust standard errors, which were used to control heteroscedasticity (Beck et al. 1993; White 1980), are in parentheses.
*$p < .05$ (one-tailed test).
**$p < .01$ (one-tailed test).

unlikely scenario? Perhaps, but there are precedents for such quick and drastic changes in governmental institutions. Though it falls outside the period of our analysis, the example of the downfall of Chilean democracy with the overthrow of Salvador Allende and the installation of the extremely authoritarian Pinochet regime represents exactly this phenomenon. In 1973, Chile achieved the maximum democracy score on the Freedom House measure of democracy, while in 1975, the next year for which the Freedom House measure is available, it achieved the minimum democratic score![33] The collapse of Weimar democracy with its catastrophic aftermath might well be another concrete example of such a drastic change.

With regard to population-related determinants of human rights abuse, the findings are mixed. In both sets of statistical analyses shown in the Table 1, the relation between population size (logged) and the repression measure derived from Amnesty International reports indicates that the larger a country's population, ceteris paribus, the greater the state's tendency to violate the integrity of its citizens through the use of repression and terror. Judgments regarding the substantive importance of the size of the coefficient would, however, vary depending on one's viewpoint. These findings are at least moderately important when viewed from the perspective of theory. One who emigrated from a country with a tiny population of about 200,000 (e.g., the Bahamas during the eighties) to one the size of the People's Republic of China would likely find, ceteris paribus, that levels of repression were much higher, since the lagged effects of population become asymptotic at around 1.4 and 1.7 in the analyses with the Freedom House and Vanhanen measures of democracy, respectively.[34]

FIGURE 1

Increase in Repression Due to Loss of Democracy



Note: Change in repression score in any year shows the direct effect of absence of democracy in that year and the cumulative lagged effects of loss of democracy on repression in the previous years, for a country that in year t changed from the most to least democratic score on the respective (Freedom House or Vanhanen) democracy index. Based on unrounded coefficients.

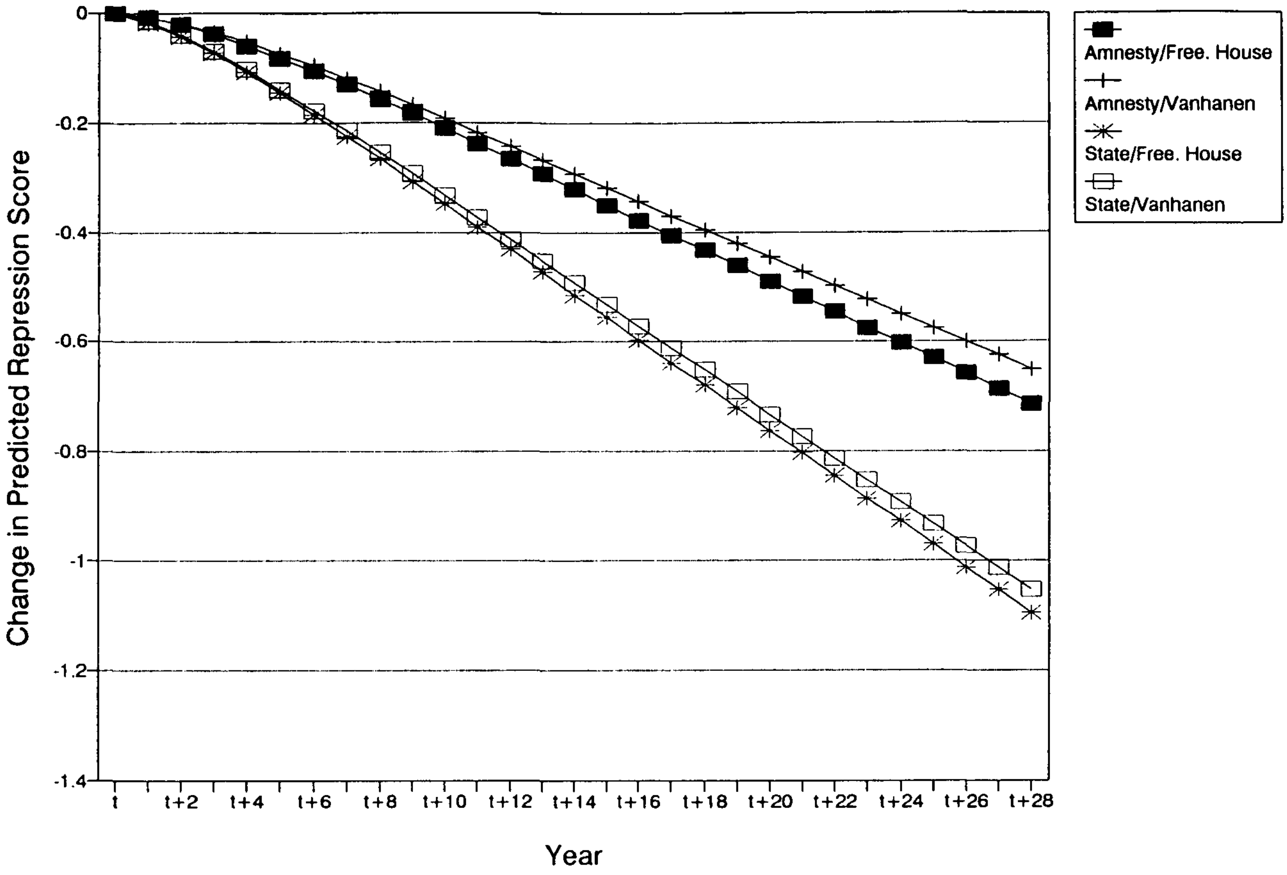
In contrast, the sizes of the coefficients are probably rather unimportant from the perspective of those seeking to conduct policy-relevant research, who would probably focus on changes in the dependent variable that result from achievable changes in the values of manipulable independent variables. When we examine the differences in repression that result from the addition or subtraction of population from countries of various sizes, the impacts seem rather small. For example, the impact of adding one million people to a population the size of China's in year t would result in an increase of less than .0002 in the repression index at time$_{t+10}$, in both sets of analyses. In the unlikely event that one million people were added to a country about the size of the Bahamas, certeris paribus, the increase in the repression index would only be around .3 at time$_{t+10}$, regardless of which measure of democracy we might use.

We find no support for the conclusion reached by Henderson (1993) that rapid rates of increase in population lead to political repression. In the set of analyses utilizing the Freedom House democracy variable (model 1), a weak and statistically insignificant positive coefficient is the result, while in the second set of analyses, using the Vanhanen democracy variable (model 2), the coefficient is weak, statistically insignificant, and negative.

The results yielded by these analyses are also mixed with regard to the effects of economic variables on national propensities toward the violation of personal integrity rights. The hypothesis linking economic growth rates to repression is not supported by the results of either model 1 or model 2. Our findings do indicate that economic standing, as measured by per capita GNP, has a statistically significant effect on such violations in model 1 and falls just short of statistical significance in model 2. But here an examination of the magnitude of the coefficient leads us to the conclusion that the effect is rather weak, since the coefficients indicate that a relatively unlikely increase of ten thousand dollars in a country's per capita GNP from one year to the next would translate into a rather small decrease in the tendency for abuse of personal integrity of between .07 and .08 on the five-point human rights abuse index, ceteris paribus. If that country's per capita GNP remained stable at ten thousand dollars above the original value, this

FIGURE 2

Decrease in Repression Due to Increase in Economic Standing



Note: Change in repression score in any year shows the direct effect of the level of GNP/Capita in that year and the cumulative lagged effects of gains of $1,000 each year on repression in the previous years, for a country that began in year t at the poorest GNP/Capita level. Based on unrounded coefficients.

would translate to a modest decrease of .24 or .22 in the repression scale at time$_{t+5}$.

If one optimistically assumes for the world's poorest country at time$_t$ a GNP per capita increase of one thousand dollars per year for a period of time long enough to make the income of that poorest country equal that of the richest country at time$_t$, the projections are those represented in Figure 2. They show that after the 28 years this change would require, the decrease in repression due to such an impressive advance in economic standing would be expected to range from about .7 or .8, depending upon the measure of democracy used, with the personal integrity abuse index derived from Amnesty International reports. These truly long-term effects would represent important decreases in repression, in the unlikely event that a nation could sustain such increases in economic development. Nevertheless, they are much less impressive when compared with the short-term effects of the loss of democracy or the onset of civil or international war.

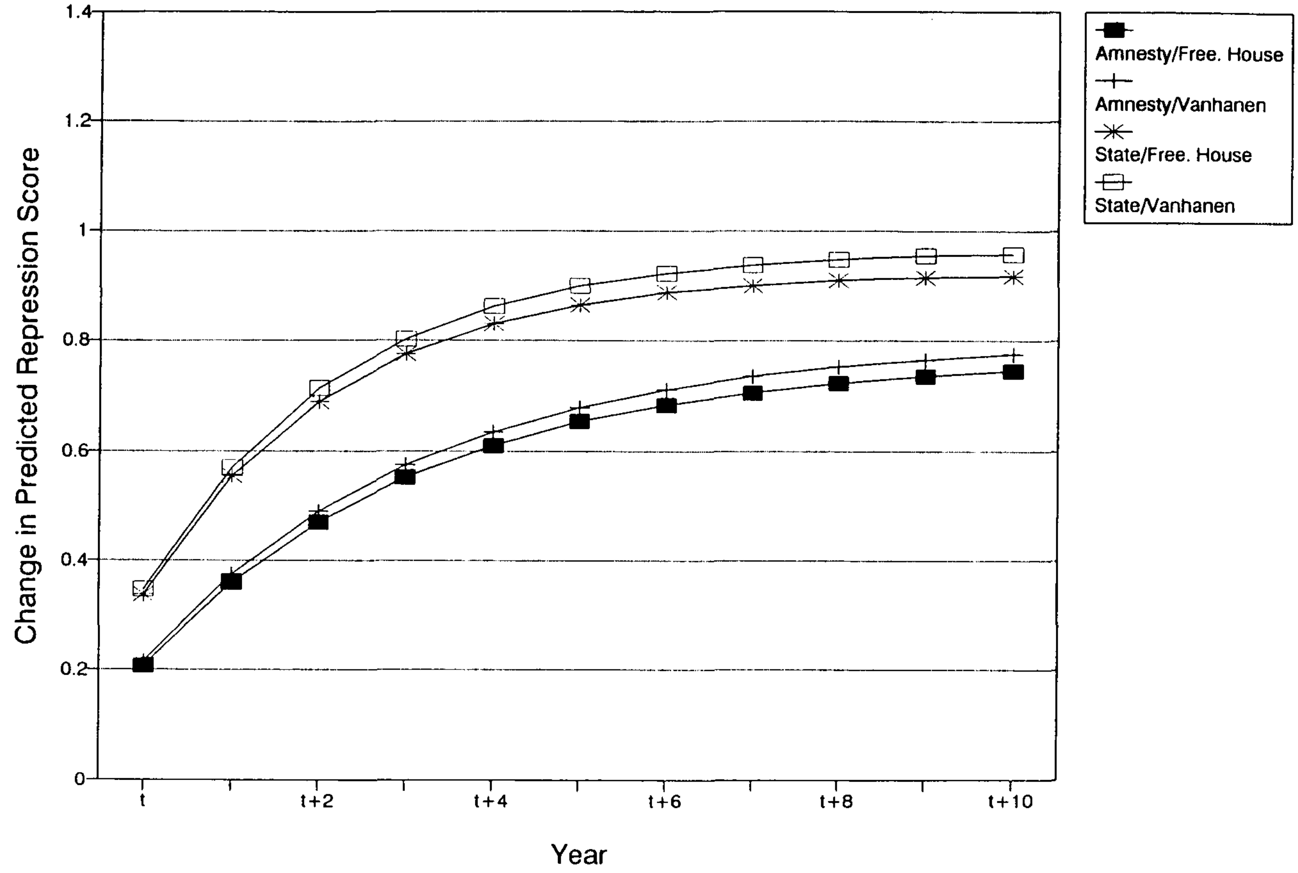
The variables identifying particular types of regimes thought to have an important impact on levels of repression do not fare too well in these analyses. While the military control variable has coefficients in the expected direction, they are statistically insignificant. And the variable identifying leftist regimes achieves a statistically insignificant coefficient in an unexpected direction.

In contrast, the dummy variables identifying levels of external and internal threat, in the form of international and civil wars, do achieve substantively important and statistically significant coefficients, indicating that as threat increases, so does a country's propensity to use repression. The variable identifying participants in ongoing international wars achieves statistically significant coefficients of .21 and .22 in models 1 and 2, respectively. These coefficients indicate that if a country were to be involved in international war for six consecutive years, we would expect an increase of about .7 in the human rights index, other factors held constant, in the sixth year (see Figure 3).

The measure of national involvement in civil war achieves a coefficient of .33, statistically significant at the .01 level, in each analysis. Once again assuming a country's ongoing involvement in civil war, its repression scale score would have increased by just

FIGURE 3

Increase in Repression Due to International War



Note: Change in repression score in any year shows the direct effect of international war involvement in that year and the cumulative lagged effects of international war on repression in the previous years, for a country that went to war in year t. Based on unrounded coefficients.

over one point in the sixth year of that war, with the impact stabilizing at about 1.2 a few lags thereafter (see Figure 4). Thus the results we obtained when using data derived from Amnesty International reports to operationalize human rights abuse strongly support the conclusion that regimes repress political opponents when faced with threats, domestically and abroad, in the form of civil and international wars.

The R-squared of .77 yielded by both models 1 and 2 shows that the theoretical model achieves a quite respectable level of explanatory power. Thus while the possibility of misleading results emerging as a result of specification error is not precluded, we believe there is good reason for confidence in these findings.

A Second Model: Ratings Derived from State Department Reports

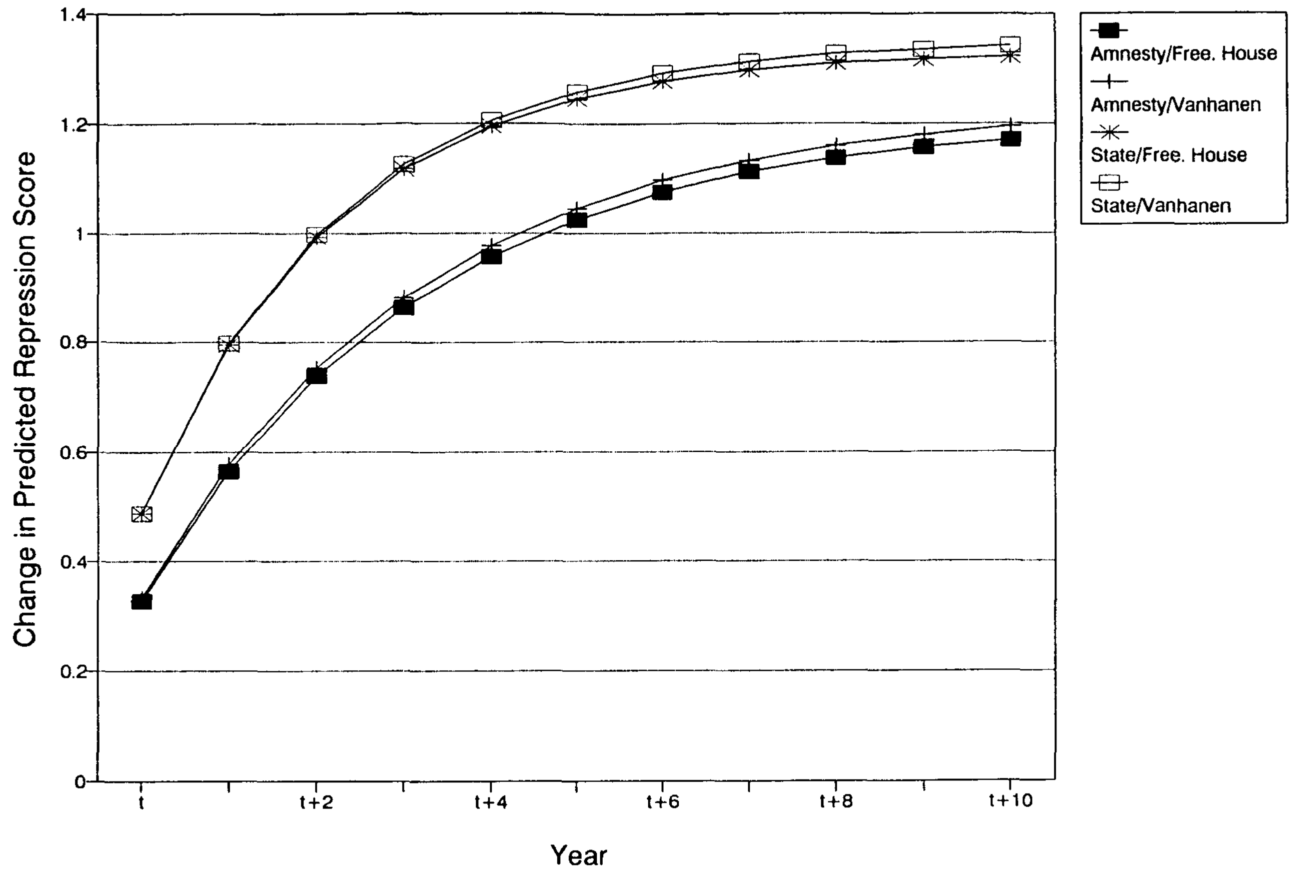
In order to check the reliability of the results obtained in our first set of analyses, we next conducted tests of similar multivariate models with an index measuring the abuse of personal integrity derived from the U.S. State Department human rights profiles. The results of these analyses are presented in Table 1: Model 3 uses the Freedom House measure of democracy, and model 4 uses Vanhanen's operationalization of that concept. The R-squared of .75 again indicate that the models achieve a respectable level of explanatory power.

The results in models 3 and 4 are very similar to those yielded when the focus was on the dependent variable generated from Amnesty International reports. The most serious divergence between this and the first set of results is that relating to the leftist government variable, which achieves strong, statistically significant coefficients in the expected direction. In our previous analyses, focusing on an index derived from Amnesty International reports, the coefficients had been extremely weak, with a negative sign. These results are precisely what one would expect if indeed the State Department's profiles are biased against leftist governments (or, alternatively, if Amnesty International's ratings are biased *in favor* of leftist regimes and movements).

The list of variables exhibiting statistically significant effects is otherwise about the same as in our tests of models 1 and 2. The variables identifying British

FIGURE 4

Increase in Repression Due to Civil War



Note: Change in repression score in any year shows the direct effect of civil war in that year and the cumulative lagged effects of civil war on repression in the previous years, for a country that began a civil war in year t. Based on unrounded coefficients.

cultural influence, and military regimes again have weak and statistically insignificant coefficients. The same can be said of the population and economic growth variables, which also failed to exhibit important effects in our initial tests. But the impacts of many of the variables found to be important in models 1 and 2 appear to be somewhat larger in this set of analyses. Economic standing, democracy, international war, and civil war each have somewhat larger effects in these models, while these of the population variable were larger in one of the two sets of analyses, and identical in the other.

A Comparative Look at Dynamic Effects

Now that we have conducted four sets of analyses, we are in a better position to assess the importance of the variables we hypothesized would affect levels of repression. With regard to the variables found to have statistically significant effects in each of the four sets of analyses, it is evident that population size, economic standing or development, international war, civil war, and democracy affect levels of repression. In Figure 1 we show the impact that the abandonment of democracy would have on personal integrity abuse, assuming a change from the most democratic score at time$_{t-1}$ to the least democratic score at time$_t$, stabilizing at the least democratic score throughout the period. While the estimates provided by the Freedom House variable are somewhat more cautious, it is clear, nevertheless, that the loss of democracy increases the abuse of personal integrity, especially when one considers its lagged effect.

The same can be said of international and civil wars, whose effects over time are plotted in Figures 3 and 4. Put in terms of the scale of the dependent variable, the results plotted in these figures show that we should expect that a country under the rule of law at the time of initiation of either an international or civil war would likely have limited political imprisonment by the end of the period, other factors held equal. A regime that already had limited political imprisonment but few more serious violations, would be expected, ceteris paribus, to hold a more extensive number of political prisoners and perhaps to torture, execute, or murder its political opponents if those wars continue.

By way of contrast, even the strongest plausible projections based on the effects of economic standing and population size seem weak when compared to the effects of the democracy and international and civil war. Generously plausible assumptions about changes in population size yield at best only modest increases in predicted abuse of personal integrity right, ceteris paribus, when projected over time. Similarly, the most optimistic estimate of economic standing's effect is that an increase of ten thousand dollars in per capita GNP, held constant for several years thereafter, years would decrease state terrorism by .15 at time$_t$, by .39, at time$_{t+5}$, stabilizing at .42 a few lags thereafter. So even though these analyses do show that economic development makes a statistically significant difference in propensities to use state terror, the size of that difference is rather small. On the other hand, given the long research tradition that has found strong correlations between economic development and democracy,[35] it is notable that both economic development and democracy have statistically significant effects on repression of personal integrity rights, regardless of the magnitudes of their impacts.

SUMMARY AND CONCLUSIONS

In this study we sought to explain variations in cross-national respect for the subset of human rights known as personal integrity rights, through the use of sophisticated multivariate methods, on a comprehensive pooled cross-sectional data set covering the years 1980–87. A model explaining variations in national respect for these rights was constructed to test several hypotheses drawn from a variety of theoretical perspectives on why such human rights violations might occur. Two distinct dependent variables, derived from information on human rights conditions provided by Amnesty International and the U.S. Department, and two indicators of the key concept of democracy were used in four sets of analyses. Because the tenor of the findings yielded by the four models is very similar, we can conclude with some confidence that the effects we isolated are in fact an accurate representation of reality.

Several of our hypotheses were supported in each of the four sets of analyses that we presented. As expected, democracy was shown to be associated with a decreased incidence in repression, regardless of which of the two indicators of that concept—and which of the two dependent variables—was employed. These results are important because they substantially extend the findings of Henderson (1991, 1993) with different measurements of democracy. As a result of the cumulation of findings on this linkage, then, it would now seem difficult to deny that democratization decreases governments' use of coercion to abuse the human rights of private citizens.

Following Henderson (1993), two population variable were employed in our multivariate model of human rights abuse. With regard to the logged population size variable, the results were stable and quite clear, and we can therefore conclude that population size does have a positive impact on human rights abuse, with more populated countries having a greater propensity to abuse personal integrity rights, ceteris paribus.

Our findings regarding several other variables thought to be related to state terror were negative. None of our results supported the conclusion that military control or British cultural influence affects levels of repression.[36] Results for the effects of population growth were stable across all of the analyses that were conducted, indicating no statistically significant or substantively important impact on repression. This finding contradicts the conclusions of Henderson (1993), which he based on his analysis of a cross-sectional data set covering the year of 1985. However, here we should note that we dealt with only a seven-year time frame and that the population increase variable utilized in this study was an average of the population gain over this seven-year period, so that unlike other variables in the model (with the exception of British cultural influence), it did not vary across time. A study using accurate yearly estimates (if such estimates are possible) conducted over a longer time frame might yield more positive results.

Rather mixed results were yielded by the variable identifying leftist governments. Leftist regimes appeared to violate personal integrity rights more seriously than others when such rights were operationalized using information gathered from U.S. State Department sources. When our analysis focused on the measure derived from Amnesty International information, however, no such relationship appeared. Here we are inclined to give less weight to the results obtained with the index derived from State Department reports, because our findings are consistent with those that would be expected if allegations regarding the biases of the State Department reports against (or of Amnesty International in favor of) some leftist regimes are valid (e.g., Innes 1992). Thus we cannot conclude that, ceteris paribus, doctrinaire socialism is a cause of coercion that abuses human rights; but the results do perhaps provide limited empirical evidence of the different "pictures" of human rights realities painted by these two sources of human rights behavior during the 1980s.

Our study does provide very firm support for the hypotheses linking national experience of international and domestic threats, in the form of international and civil wars, to an increased tendency to abuse personal integrity rights. Variables identifying countries that were participants in both kinds of wars were found to have statistically significant and substantively important impacts on national respect for the personal integrity of citizens in each of the four sets of analyses, with civil war participation having a somewhat larger impact than participation in international war. Thus this study is the first to document the impact of involvement in violent conflicts, in both the international and domestic arenas, on levels of repression around the world.

Finally, with regard to economic explanations of human rights abuse, we found in each of our four sets of analyses that economic standing is negatively, but only rather weakly, related to regimes' propensities to abuse of personal integrity rights.

Having now nearly finished this investigation of the conditions that lead to the most serious forms of human rights abuse, we believe we would be remiss if we failed to consider the vitally important question of what we have learned that might be helpful to scholars and practitioners hoping to decrease abuses of personal integrity worldwide. While our primary focus in this study has been on theory, our findings do speak to issues of interest to practitioners. If governments, international organizations, and subnational actors are interested in improving respect for personal integrity around the world, one way for these actors to make headway toward this goal is, not surprisingly, to promote democracy. Promoting democracy is clearly not the entire answer, however. Our results indicate that if a nondemocratic country known to be a very serious violator of personal integrity rights were to change to a democratic form of government, human rights abuse would probably not disappear completely, other factors being equal. Neither should economic development be viewed as the panacea. Efforts to improve economic conditions within a country through programs like foreign aid might at times successfully promote human rights, defined more broadly, by leading to the provision for basic human needs, but our study indicates these conditions have, at best, a moderate impact on respect for personal integrity. Neither would population control efforts be likely to have a substantively important impact on repression of these rights.

Our findings do, however, reveal another course toward greater respect of personal integrity, one that would have an impact of about the same magnitude as converting autocratic regimes to democratic ones. We have shown that these basic rights can be enhanced by actors who would encourage countries to solve their political conflicts short of war, and use whatever means are at their disposal to assist them in doing so.

APPENDIX

In order to add to the already extensive data set gathered by Stohl and his colleagues (n.d.), Poe and Sirirangsi (1993, 1994) analyzed the content of the State Department and Amnesty International reports for the years 1980–87 and assigned values on a five-point ordinal scale according to the following coding rules, originally set forth in Gastil (1980):

1. Countries [are] under a secure rule of law, people are not imprisoned for their views, and torture is rare or exceptional. . . . Political murders are extremely rare.
2. There is a limited amount of imprisonment for nonviolent political activity. However, few persons are affected, torture and beating are exceptional. . . . Political murder is rare.
3. There is extensive political imprisonment, or a recent history of such imprisonment. Execution or other political murders and brutality may be common. Unlimited detention, with or without trial, for political views is accepted.
4. The practices of [level 3] are expanded to larger numbers. Murders, disappearances are a common part of life. . . . In spite of its generality, on this level terror affects primarily those who interest themselves in politics or ideas.
5. The terrors of [level 4] have been expanded to the whole population. . . . The leaders of these societies place no limits on the means or thoroughness with which they pursue personal or ideological goals. (Gastil 1980, quoted in Stohl and Carleton 1985)

Following Stohl and his colleagues (n.d.), two coders read and analyzed the content of each source and assigned a number between 1 and 5 to each country. After all coding was completed, the scores assigned by the two coders were compared. It was found that intercoder agreement was 85.6%, a percentage a bit lower than that achieved in one similar human rights data-gathering project (McCormick and Mitchell 1988, 1989) but still quite respectable. This percentage and the gamma statistics for the two variables indicate high intercoder reliability. For the measure derived from the Amnesty International reports, the gamma was .94; for that derived from the State Department profiles, it was .98.

After compiling the scores, the coders found that disagreements often occurred on three kinds of cases. First, there were cases that were categorized as a 2 by one coder and as a 3 by the other. These tended to be countries in which there was limited political imprisonment but considerable alleged torture or in which there was more torture than would ordinarily be indicated by the wording for the 2 category. A second kind of disagreement occurred on cases in which prisoners were taken after alleged demonstrations, riots, or revolution attempts but with uncertainty over whether their political activity was violent or not. Since a major concern was to make our data comparable to those gathered for use in the Stohl study, we consulted those researchers to see how they had dealt with these cases. A telephone conversation with Mark Gibney (February 1991) confirmed that those researchers gave the benefit of the doubt to the government, following the dictate that they should be "innocent until proven guilty." To remain consistent with the data provided by these researchers, we, too, followed this decision rule.

A third type of case on which coders tended to disagree involved countries that imprisoned conscientious objectors identified by Amnesty International as "prisoners of conscience." In such cases, according to Gibney, countries with large numbers of imprisoned conscientious objectors were coded as being in the 2 category as long as other human rights difficulties did not exist, but those that had only one or two such prisoners were coded as being in the 1 category. Again, following the practice of these researchers, most of these disagreements were resolved through discussion and reconsideration of the facts of the particular cases. But if no resolution was in sight, again following the practice described by Gibney, a third coder was brought in to cast the deciding judgment.

Notes

We could not have written this article without the help of our colleagues and students. Comments and advice from Harold Clarke, Neal Beck, Jim Meernik, and Motoshi Suzuki kept us on track in our analysis of the pooled cross-sectional data set. The editorial suggestions of Vernon Van Dyke added grace and clarity to our sometimes inelegant language. Mark Gibney, Michael Stohl, David Carleton, and their coresearchers at Purdue University generously shared the human rights data they had coded from the Amnesty International and State Department reports for 1980 to 1987 and graciously answered inquiries about the coding of these data. The Interuniversity Consortium for Political and Social Research provided machine-readable versions of the data series contained in the World Bank's *World Tables*, in several volumes of Ruth Leger Sivard's *World Military and Social Expenditures*, and in the Freedom House *Freedom in the World* series, all of which provided portions of the data used in the analysis. These data are available to interested parties via GOPHER (gopher.unt.edu, port 70): look under "UNT Information & Resources/Departments, Schools & Colleges Information/College of Arts and Sciences/Political Science/Repression." They are also available via anonymous FTP (ftp.unt.edu.)

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1. The focus of our analysis will be the abuse of internationally recognized human rights, specifically those having to do with integrity of the person (Cingranelli and Pasquarello 1985; Henderson 1991, 1993; Mitchell and McCormick 1988; Stohl and Carleton 1985), a choice that will be discussed and justified later. We will, however, also refer to the dependent phenomenon as *state terrorism*. Our use of this term is compatible with that of Gurr, who states: "Regimes do many coercive things to induce compliance. They threaten, arrest and jail, fine and confiscate as well as murder. It is plausible, both analytically and psychologically to limit the concept of state terrorism to coercion that takes or grossly endangers the lives of its targets. Thus it includes imprisonment in conditions where many are worked or starved to death, and other denials of means of life, as well as outright killing. . . . Violence by regimes is terroristic only if it is 'instrumental', which means designed to have a wider effect on some audience" (1986, 46). We choose to employ the terms interchangeably because the two phenomena are inextricably linked: abuse of individuals is a human tragedy directly resulting from terrorist policies employed by nation–states.

2. For the linkage of U.S. foreign aid allocation to human rights issues, see Carleton and Stohl; Cingranelli and Pasquarello 1985; McCormick and Mitchell 1988, 1989; Poe 1990, 1991, 1992; Schoultz 1980, 1981; Stohl and Carleton 1985; and Stohl, Carleton, and Johnson 1984. For respect for human rights in relation to U.S. refugee policies, see Gibney, Dalton, and Vockell 1992; Gibney and Stohl 1988. For a linkage between democratic practices and national tendencies to go to war, see Rummell 1983.

3. McKinlay and Cohan analyze a set of regimes and regime-years across a 20-year time span, but they do not explicitly build time or within-unit change into their work.

4. Violations of personal integrity are usually perpetrated directly by government officials and thus are more easily dealt with by a change in government policy, in direct contrast to violations of economic and social rights, which are often less amenable to change. It is unfortunately doubtful that economic rights (e.g., a right to food, a job, and a home) will be guaranteed in many African countries in the near future even if the government's intentions are exemplary, since the causes of these problems are oftentimes not in the government's control. Respect for social rights (e.g., social equality for women and minorities) are often based on centuries of cultural practice. For a regime to attempt a sudden change in practice before the time is right might lead to a coup, a revolution, or civil war. In such circumstances even reform-minded regimes will likely be slow to change, for to change too quickly could result in a serious setback for the causes they are attempted to promote. By focusing our study on rights associated with the integrity of the person we are purposely directing our inquiry toward policies and crimes we feel can more easily be altered by national governments in the short term, in an effort to be policy-relevant. Perhaps in our future work we will deal with other definitions and dimensions of human rights.

5. For arguments that better data are needed, see Jabine and Claude 1992, esp. chapters by Banks, Bollen, and Lopez and Stohl. However, statements in some of these chapters and empirical work by some of these same researchers (e.g., Stohl and Carleton 1985; Stohl, Carleton, and Johnson 1984) support the argument that current data are sufficiently good to be used in meaningful empirical tests.

6. The measurement alternatives currently available to researchers of human rights phenomena are outlined and discussed in Lopez and Stohl 1992 and Stohl et al. 1986. Events data measures, like those reported in the *World Handbook of Political and Social Indicators* (Taylor and Jodice 1983), depend on newspaper accounts of different human-rights-related phenomena and use counts of state coercive behaviors (e.g., the imposition or lifting of government "sanctions" and political executions) to generate measures of human rights abuse. Research using events data to operationalize various kinds of repression has been reported by a number of scholars (Alfatooni and Allen 1991; Davenport 1992; Davis and Ward 1990; Hibbs 1973; Muller 1985; Ziegenhagen 1986).

Stohl and his colleagues (1986) cite major problems with this approach. First, the different repressive behaviors counted are often substitutable means used to reach the same end—the repression of those who are thought to pose an obstacle to the policies or continued power of the incumbent regime. For example, a decrease in reports of politically motivated arrests by a government might not be an accurate indicator of improving human rights if that decrease takes place soon after several hundred opponents of the government were executed. Second, since reports of human rights abuses are routinely suppressed by closed regimes to an extent that would not be possible in more open societies, events data measures sometimes lead to counterintuitive and invalid findings, like that reported by Stohl and colleagues indicating that the United States was a more repressive society in the years 1948–50 than the Soviet Union under Stalin. We therefore are in strong agreement with the pleas of Stohl and colleagues, who argue that "we must bring political knowledge to bear in shaping our collection of information and measurement schemes" (p. 598). So while events data may provide a useful supplement to the approach we take in this research, we choose not to use it primarily for the reasons just rehearsed. In addition, the events data we would need to operationalize our dependent variables are simply not currently available and our comprehensive international sample for our time period.

Accordingly, we see a standard-based approach as the best available measurement alternative at this time. An advantage of this approach is that it allows for a greater degree of political knowledge to be used in the development and application of standards to sources of human rights information specified by the researchers. Further, such measurement techniques allow us to focus clearly on the integrity-of-the-person rights in which we are most interested in this study.

7. The ratings were derived from country profiles included in the Amnesty International Reports for 1981–88 and the State Department's *Country Reports on Human Rights Practices* for 1981–88.

8. In coding the Amnesty International profiles, the least repressive coding of 1 was assigned in only 71 cases for 1980–87, compared to 376 cases for the State Department reports. But there was little difference between the two in the number of cases their reports led us to place in the four higher categories. Further, of the 302 cases that were missing in Amnesty International, we gave 220 codes of 1 in our ratings derived from the State Department reports. Therefore, while Amnesty insists that the "omission of a country entry should not be interpreted as indicating that no human rights violations took place in that country" (Amnesty International 1987, 2), in practice, those countries that were omitted from the Amnesty International reports tended to be (but were not always) countries that appeared to adhere to the rule of law.

9. An issue of special concern to us are allegations of bias against the State Department reports (e.g., Carleton and Stohl 1987 and the sources cited therein). However, it is also important to note that some argue that the consistency of the U.S. State Department reports improved during the 1980s and therefore may be more valid in that period than they had been previously (e.g., Innes 1992). Furthermore, if some have argued that the State Department has been biased against leftist regimes, others have suggested that Amnesty International may have been more likely to note abuses by governments against armed leftist oppositions than by those oppositions against government and its supporters.

10. Closely related to these arguments are those of Howard and Donnelly (tested in the Mitchell and McCormick study) that "internationally recognized human rights require a liberal regime" because such regimes give the individual priority over the state (1986, 802). Since liberal regimes clearly fall into the subclass *democracy* as that term is conventionally defined, we see no reason to represent them separately in our analysis.

11. For example, the preface to a recent "worldwide survey" of democracy, notes that "humanitarian concern for human rights is inseparable from . . . democracy or the lack of it. Human rights are part, a good part, of democracy, and they can be assured only by legal, responsible, that is, democratic governments—at the same time affirming that "the question of democracy is broader" than the question of the status of human rights as typically assessed by Amnesty International, the State Department, and Freedom House (Wesson 1987, vii).

12. Bollen (1980) went on to create a variable based on his definitions, covering the years 1960 and 1965. Unfortunately, the measure has not been replicated on an annual basis and thus cannot be used in this study. For further discussion, see Bollen 1990, 1993.

13. The Polity II operationalization of institutional democracy omits any consideration of whether a regime respects the dignity of the person, which makes it an ideal measure for our purposes. Unfortunately, the Polity II measure is available only through the year 1986 and for only 127 of the maximum of 153 nations for which we have measures of state terrorism and our other independent variables. Since we lacked the time and resources to expand the Polity II democracy data to cover 1987 for all nations and 1980–86 for 26 additional nations, using Gurr's democracy index would have substantially reduced the N for our pooled analysis, so we explored other alternatives. However, analyses conducted with the Polity II measure on this reduced sample led us to conclusions similar to those reached in the analyses we shall present. The former are not presented in more detail due to space limitations.

14. A summary collection of values for this variable is reported in *Freedom in the World* (McColm 1990).

15. We do not wish to argue that the Freedom House indicators cannot be used to measure human rights, very broadly considered. But, as we have explained, the indicators derived from the Amnesty International and State Department reports, which we analyze, focus much more narrowly on those infringements of the dignity of the person that we and others label "repression."

16. The criteria quoted from the 1989–90 edition of *Freedom in the World* (McColm 1990), are more elaborate than those in earlier editions. Nevertheless, the criteria, if not the clarity with which they have been expressed, appear to have been consistent over the years. Compare, for example, the following from the 1980 edition of *Freedom in the World*: "*Political rights* . . . allow people to participate freely and effectively in choosing their leaders or in voting directly on legislation. . . . The rating a nation receives for political freedom is determined by factors such as the existence of two or more competing political parties or the independence of opposition candidates from government control. . . . Elections and legislatures have to demonstrate a significant opposition, and those elected have to be given real power" (Gastil 1980, 4–5; emphasis original).

17. These freedoms and others are a part of the Freedom House civil liberties rating. However, the civil liberties rating is not a viable measure of democracy for our use because it specifically includes "protection from unjustified political terror, imprisonment, exile or torture," which overlaps clearly with the indicators derived from the Amnesty International and State Department reports, which are our dependent variables, as well as a number of other rights that are not essential to democracy as envisioned by Bollen (1990, 20–21). Perhaps because the Freedom House civil rights indicator includes basic civil liberties that are a part of many definitions of democracy, the overall correlation between the political and civil rights ratings in our data set is very high, .935. Nevertheless, the conceptual differences between the two indicators and the explicit overlap of the civil liberties indicator with repression convinces us to consider only the political liberties rating as our measure of democracy.

18. This was also noted by Gurr. In contrasting the Polity II indicators with the Freedom House measures, Gurr noted that "the problem with the latter is that Gastil assigned annual scores partly on the basis of repression" (personal communication, 15 September 1992).

19. Bollen's (1993) article appeared too late for us to take full account of his findings and his measurement suggestions in our analysis. But we should note that despite his conclusion about the superior validity of the Freedom House measure, Bollen does not endorse the use of a single indicator. Instead, he suggests using structural equation models to construct indicators that maximize validity while minimizing systematic and random measurement error. Unfortunately, he also notes that techniques appropriate for dealing with "pooled cross-sectional and time series data for nations" are "underdeveloped in latent variable models" (p. 1224). As a secondary strategy, he suggests using factor scores from several indicators as a composite measure of democracy. We might have adopted this approach but did not because (1) the data required to construct the composite index ultimately recommended by Bollen were not available to us in appropriate time series form prior to our completion of our analyses and (2) the suspicion with which many analysts regarded the Freedom House measure mandated that we analyze it separately in order to contrast it with an indicator that represented a different and nonjudgmental approach to measuring democracy, the Vanhanen index.

20. The zero-order correlations between the Freedom House measure of democracy and indicators of human rights abuse are only −.47 for ratings derived from the Amnesty International reports and −.54 for those derived from the State Department profiles in the 1980–87 period. With the Vanhanen measure of democracy, the correlations were −.50 and −.52, for those two measures of human rights, respectively. With the Polity II measure, which limits the analysis to a smaller and temporally different number of cases the correlations were −.55 and −.52, respectively.

21. The skewed distribution of total population made it desirable to log this variable to meet the statistical assumptions of our methods.

22. Clearly, the ideal would have been to employ estimates that accurately reflect changes from year to year. When efforts were made to obtain such estimates, however, the results were disappointing. The numbers generated for many data points were simply unrealistic, due to sudden changes (sometimes unexplainable decreases) in gross population estimates for Third World nations and for countries with small populations. We therefore employed a variable indicating the average population change of a country over the eight-year period of the study as the measurement alternative that was better in practice. We did not expect—and saw no—evidence of faulty estimates causing any difficulties in estimating the gross population variable, as the few difficulties were not of a difference of magnitude.

23. For example, Henderson used energy consumption and its growth as his measures of economic growth and development, and Park used the physical-quality-of-life index created by Morris (1979) as his measure of "economic basic needs rights." In addition, Henderson used an international human suffering index to measure "socioeconomic needs," which turned out to be very highly correlated with his energy consumption measure of economic development (Population Crisis Committee 1987). The continuing dissatisfaction with GNP per capita has also led to the creation and reporting annually of a new human development index by the United Nations Development Programme (1990, 1991).

24. Although this classification was created by a single coder, Hamed Madani, it has been subjected to a variety of tests designed to document its validity and reliability. Cross-verification against the coup data set of O'Kane (1987) and against the "military control" classifications of Sivard (1989) found very few instances in which Madani's data disagreed with O'Kane's as to whether a coup had occurred in a given country and relatively few instances when Sivard coded a nation as military-controlled and Madani did not. In the latter case, such disparities appeared to result more from the use of a broader and vaguer classification scheme by Sivard than from any error on Madani's part. In the former case, O'Kane's data usually appeared to be in error. As a final test, Madani's classification was compared to two independent classifications prepared by two undergraduate research assistants, Jessie Hill and Michael Prior. Again, there proved to be few instances of disagreement, and, when there was disagreement, it was most often because the undergraduates coded a regime as civilian due to a failure to realize that it had originally come to power through a coup.

25. Bollen and Jackman (1985, 1988) provide strong empirical evidence for the effect of British colonial influence on democracy, as operationalized by Bollen's democracy index for 1965. However, readers should remember that like Mitchell and McCormick, we take pains to distinguish democracy, one of our independent variables, from repression of personal integrity, our dependent variable.

26. Using events data in a crossnational research design, Hibbs does show a positive relationship between "internal war" and "negative sanctions" by government (1973, 182).

27. In coding international and civil wars we used as a guide information from Sivard 1991 and Brogan 1990. In cases where there were doubts as to whether the criteria were met, we sought information from other sources dealing with the situation in the particular country.

28. The White technique was adapted to the PCT design by Beck and his colleagues (1993). According to them, "if X is the variance–covariance matrix, the robust variance–covariance matrix of the errors is estimated by"

$$(X'X)^{-1}X' \frac{(E'E)}{T} \otimes I_T X(X'X)^{-1},$$

where $\mathbf{E}$ is the $T \times N$ matrix of residuals, T is the number of time points, N is the number of nations, $\mathbf{I}$ is the $T \times T$ identity matrix, and $\otimes$ is the Kronecker product (p. 946). The equation

$$\frac{(E'E)}{T} = \hat{\Omega}$$

represents the estimated covariance matrix of the country errors used in feasible generalized least squares analyses of pooled cross-sectional time-series data sets, (ibid.). This procedure does not affect the OLS coefficients, but it does estimate more consistent standard errors. The RATS procedures that allowed us to use the robust-standard-errors approach on PCT data were developed and used in Beck et al. 1993 and Beck and Katz 1994 and were generously provided by Nathaniel Beck, via his FTP server.

29. To test for the possibility of autocorrelation difficulties, we computed the basic model for both the dependent variables derived from the Amnesty International and State Department reports with both democracy variables. An inspection of the residuals gained from OLS regression turned up few examples of sign changes in the residuals resulting for particular countries, a pattern indicating that positive autocorrelation is a difficulty (Ostrom 1990). The Durbin–Watson statistics ranged from .44 to .86, in the four preliminary tests of the basic model, thus also indicating that positive autocorrelation likely affects OLS results. For an argument advocating the use of lagged dependent variables to deal with autocorrelation difficulties, see Beck and Katz 1994.

30. We were reluctant to discard data from this year because it decreased our sample size by one-eighth. We therefore experimented with an estimation method that allowed us to retain the first time point while dealing with autocorrelation and heteroscedasticity–the Prais–Winsten estimation technique (Ostrom 1990, 31–41), used in a first-order autoregressive (AR(1)) model for which we calculated White's robust standard errors, using RATS 386, version 4.02. There were some divergences between the results yielded by this approach and those we rely on herein. Specifically, it appeared that military control and British cultural influence had much stronger and usually statistically significant effects on repression when the first case was retained in an autoregressive model with robust standard errors. However, when we inspected the estimates calculated for the first case for each of our variables, we became skeptical. The values of the estimates usually appeared quite atypical of their series, especially for the dichotomous variables. Also, when tests were conducted with a similar AR(1) model, with the first year discarded to ascertain the effects of these estimates, the coefficients for these variables decreased in both size and statistical significance. We took this as an indication that the strength of these variables in our AR(1) models with the Prais–Winsten estimates was likely due to faulty estimations of the first time point. In the text, therefore, we choose the more cautious path and present the analyses with the first case discarded, due to our use of an lagged endogenous variable—analyses that do not include significant effects for British cultural influence or military control.

31. Since the Durbin–Watson statistic is biased when a lagged dependent variable is entered on the right side of the equation, the autocorrelation functions were examined to determine whether the autocorrelation difficulties were solved. The autocorrelation functions for lag 1 in the four sets of analyses ranged from −.16 to −.18, with standard errors of about .03. Most of the autocorrelation functions for the remaining lags were at, or below, .10, the largest being just .18. Thus we can say with some confidence that autocorrelation does not pose a problem in our interpretation of these results.

32. This can be done quite simply. To find the effect of the loss of democracy at time$_{t+1}$, we simply multiply the effect of this loss at time$_t$ by the coefficient of the lagged dependent variable and add the direct effect of democracy at time$_{t+1}$. Repeating this process for each successive lag will, after several lags, fall into an asymptotic pattern if the loss of democracy continues.

33. The Vanhanen democracy index is not available for the 1970s.

34. We do not present graphs of these effects here, but we will gladly provide them on request.

35. This literature is far too voluminous to cite fully. Relevant early research includes Cutright 1963; Lerner 1958;

Lipset 1959; and the studies cited in Tate 1972. More recent work is cited and discussed in Bollen 1990, 1993; Bollen and Jackman 1985, 1988; and others.

36. However, in a set of supplementary analyses for 1981–87 using an AR(1) model to control autocorrelation and White's robust-standard-errors approach to control for heteroscedasticity, the British influence variable did exhibit a statistically significant coefficient of moderate strength (.2 to .3) in analyses using the Vanhanen measure of democracy. So perhaps this hypothesis should not be dismissed completely at this point. Other results yielded by these analyses were very similar in terms of the variables that reached statistical significance, with the exception of the international war variable, which failed to reach conventional levels of statistical significance in analyses with the dependent variable derived from Amnesty International profiles, but did achieve statistically significant coefficients of around .5 in the analyses based on ratings generated from the State Department reports. These analyses are not presented in more detail because of space limitations and because the findings are generally similar. We decided to present the OLS analyses with the lagged dependent variable because it is a more widely understood method, because of the ease in calculating the lagged effects of independent variables, and because the results were more consistent from one set of analyses to another. The results of the AR(1) models described here are available upon request from the authors.

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"THE SLOW BORING OF HARD BOARDS": METHODICAL THINKING AND THE WORK OF POLITICS

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From the perspective of quite different theoretical traditions, Simone Weil and Hannah Arendt present nearly identical and equally powerful critiques of technological determinism in modernity. Yet unlike Arendt, Weil develops a concept of work that draws a distinction between technology and instrumental action as "methodical thinking." As a result, Weil's theory of action embraces something that Arendt's theatrical politics rejects—a concept of liberatory instrumentality, or purposeful performance. I shall reassess some of the inadequacies of Arendt's concept of work and develop Simone Weil's concept of methodical thinking in order to argue for a more neighborly affinity between work:interaction and purposeful:theatrical performance than Arendtian public realm theory, for all its power, currently allows. If such an affinity is possible, then public realm theory might be more adequately equipped to deliver on what I take to be its promise as an emancipatory project in late modernity.

> When we were forming the new structures, we had to hire people from the old structures. Our supporters—the people who came to rallies and street demonstrations—didn't know anything about how to run a country.
>
> —Aleksandr Sokolov, ex-commandant
> Central Committee Building, USSR

In 1994, we are just beginning to take the measure of the astonishing revolutionary events that heralded the final decade of this century and initiated the capacity of millions of citizens to act in concert for the power of democratic self-realization. For political theory, the challenge in the aftermath of 1989 is to advance understandings of democracy and political life that attempt to recover an emancipatory potential in modernity and meaningfully confront what Václev Havel calls "the practical task of organizing a better world" (1990, 136). In many respects, I think that the most promising source for this enterprise lies in public realm theory, perhaps particularly in the theories of Hannah Arendt and Jürgen Habermas. Both of these thinkers sustain a deep critical commitment to a politics of citizen interaction, reciprocal understanding, self-realization, and human dignity in a realm where, as Arendt puts it, "speech and only speech [makes] sense" (1958a, 25–26).

The compelling quality of Arendt's and Habermas' public realm conceptions of politics has much to do with their adaptations of a venerable conceptual strategy that has its roots in Aristotle and draws a palpable distinction between the human activities of "work" (*techne*) and "interaction" (*praxis*). With this strategy to hand, public realm theory inventively juxtaposes a theatrical image of the space of appearances (Arendt) or a communicative metaphor of the public sphere (Habermas) against an instrumentalist reading of manipulative work-processes. Thereby, Arendt and Habermas effectively particularize politics as a vibrant and unique human activity and develop a critique aimed at freeing the subject from the technical domination and violence to nature that they resolutely associate with work. They also invest freedom or emancipation in expressly noninstrumental, linguistically mediated, modes of human action in the world.[1]

My inclination is to support attempts to locate freedom in a politics of speech or communicative interaction. Thus I do not wish to contest the turn in public realm theory that allows intersubjective understanding to achieve, as Axel Honneth puts it, "the status in the theory of emancipation which social labor had had in Marxian theory" (1982, 45).[2] Nevertheless, I think that the conceptual strategy that Arendt and Habermas so brilliantly employ in order to counter modern theories of action as social labor is a double-edged sword; for even as the conceptual dichtomy between work and interaction provides public realm theory with a valuable analytical device for distinguishing between human activities, it undercuts the effective theorizing of politics as a human practice. The dichotomy *work:interaction* seems especially to foreclose what Max Weber had in mind when he called politics "the slow boring of hard boards"—a sustained, purposeful activity that meets obstacles and undertakes acts of transformation in the world (1946, 128). This conceptual inadequacy in public realm theory is not, I think, a minor matter. If our most praxis-oriented theories cannot deliver an action-coordinating concept that appreciates the purposeful nature of human struggle as politics, we should hardly be surprised to find the "project of modernity" (Rasmussen 1990, 7–8) deviously undermined, if not defeated, by other alternatives, including the seductive, "affirmative nihilism" (Villa 1992, 719) of late-twentieth-century postmodernism.

We need, then, to rethink the action concept of politics in public realm theory. To pursue this task, I shall focus explicitly on Arendt's theory of action,[3] not only because it presents public realm theory in what is perhaps its most exemplary anti-instrumentalist form but also because it is has played a role in at least one of the most dramatic emancipatory events of our time.[4] To provide an instructive critical perspective, I turn to a contemporary of Arendt's, the French thinker Simone Weil. Compared to Arendt, Weil has received relatively little attention as a social and political theorist; but she is surely Arendt's equal as a diagnostician of modernity.[5] Indeed, from the perspective of quite different theoretical traditions, Weil and Arendt present nearly identical and equally powerful critiques of the technological determinism of modernity.

Yet unlike Arendt, Weil draws a distinction between technology and instrumental action that refuses to reduce all forms of instrumentality to violence or purely manipulative processes. As a result, Weil's concept of work as *methodical thinking* embraces something that Arendt's conceptual strategy rejects—a liberatory form of instrumentality. A liberatory form of instrumentality is precisely what public realm theory must be ready to accommodate if it is to be practically prepared for the challenge posed by the world events of 1989. Arendt's public realm theory in particular needs to discover a more *neighborly affinity* (in Hans-Georg Gadamer's felicitous phrase) between work:interaction and instrumentality:politics than the current agonistic/discursive debates over Arendtian politics allow.[6] With this in mind, I tentatively propose the idea of a *methodical politics,* in order to suggest that Weil's notion of work (instrumentality as purposeful performance), is potentially compatible with Arendt's notion of interaction (politics as theatrical performance). If this affinity is indeed possible, then public realm theory might be more adequately equipped to deliver on its promise as an emancipatory project in modernity.

AUTOMATISM AND THE SHATTERING OF THE WORLD

Simone Weil's and Hannah Arendt's very different theoretical assessments of freedom in modernity nevertheless arise out of nearly identical diagnoses of the times. In a very general sense, they were both part of a generation of twentieth-century social theorists who lived their formative intellectual lives in interwar Europe, in an epoch when the Enlightenment's summer bloom was past and modernity's polar night proceeding.[7] Equivocal humanists and ambivalent modernists, both Weil and Arendt bear the traces of darkness before the face of universalized forces of domination. Yet in their arguments we can also find purposes that place them within a context of twentieth-century thinkers struggling to redeem the modern project of emancipation. Weil and Arendt's respective analyses are, in this sense, efforts to respond to the sterile passivity of modernity, the degradation of the human person, and the suppression of individuality by appealing to thought–action accounts of freedom that are aimed at recovering what technological forces have taken away. Consequently, Weil and Arendt share a kind of hope in their concern to identify forms of human activity that disclose the reality of the world, so that human beings as individuals may actualize "relations worthy of the greatness of humanity" (Weil 1973, 104) and guarantee that "human life can be at home on earth" (Arendt 1958a, 134).

Like many intellectuals of their time, Weil and Arendt understand the crisis of the modern age as a period of profound fragmentation and deformation accelerated by "the frenzy produced by the speed of technical progress" (Weil 1973, 54).[8] Like others, they situate the modern crisis in relation to the aftermath of the Industrial Revolution, wherein the imperatives of economic conquest, mass production, routinized labor, and relentless consumption gradually usurp the activities of fabrication and workmanship (ibid., 115; Arendt 1958a, 124–25). More uniquely, however, they both confront what Arendt vividly terms the shattering of "the very purposefulness of the world" (1958a, 150) as the consequence of something they call *automatism.* Automatism is the third stage in human production, and the most recent feature of the productive relations of advanced, consumer capitalism (ibid., 4, 147–49; Weil 1973, 52). Automatism, Weil argues, follows a principle that "lies in the possibility of entrusting the machine not only with an operation that is invariably the same, but also with a combination of varied operations" (1973, 52). Arendt draws the implication that "we call automatic all courses of movement which are self-moving and therefore outside the range of willful and purposeful [human] interference" (1958a, 151). Assessing automatism as a complex of elements, Weil and Arendt identify the same three elements as problematic.

First, automatism (or automation) involves the appropriation of *homo faber* (roughly, work) activity by forms of mechanized industrial production that substitute reflexive, bodily processes for experiential work that distinguishes between ends and means, operation and product (Arendt 1958a, 149–51; Weil 1977). Under the pressure of automatic machines, Arendt argues, the defining features of *homo faber* are in jeopardy. The distinction between means and ends no longer makes sense; tools and instruments "lose their instrumental character"; the standards of utility and beauty are replaced by "basic functions"; the act of fabrication is subsumed in consumption; and *homo faber*'s driving attitude—the "conscious human effort to enlarge material power"—is neutralized in a stream of never-ending processes (Arendt 1958a, 137, 145, 152). As Weil notes, under automatic machines, "thought draws back from the future" into a "perpetual recoil upon the present" in which human beings are "reduced to the condition of automata" (1973, 92; 1977, 57).

Second, automatism simultaneously supplants and recuperates the rhythms of nature, thereby invoking a form of species existence that mimics the natural life cycle, elevates basic functions, and deflates the social value of human creativity and worldly artifice. As a result of technical developments, Weil writes, "humanity finds itself as much the plaything of the forces of nature, in the new form that technical progress as given them, as it ever was in primitive times" (1973, 83). Arendt elaborates: "The danger of future automation is less the much deplored mechanization and artificialization of natural life than that, its artificiality notwithstanding, all human productivity would be sucked into an enormously intensified life process and would follow automatically, without pain or effort, its ever-recurrent natural cycle" (1958a, 132). Automatism is not only an obstacle to individual freedom; it is also a specific organizational form of the human species that, paradoxically, liberates action from the constraints imposed by the pains and dangers inherent in earlier forms of life by mutating action into an extramundane repetitive process.

Within this situation, a third element arises in the complex that identifies the modern age—the dissolution of the world of the senses and appearances into a pseudoworld (Arendt 1958a, 153) that produces pseudo-ideas (Weil 1973, 110). Rather remarkably, both Weil and Arendt identify algebraic physics as the prime producer of a nongenuine, counterfeit world (Arendt 1958a, 264–73, 285–88; idem 1969, 265–90; Weil 1968).[9] Physics proceeds by stripping away the subjective effects of the human senses with a view toward an objective theory of physical particles in fields of force. It thereby manifests a loss of confidence "in the phenomena as they reveal themselves of their own accord to human sense and reason" (Arendt 1969, 272). Weil argues that for physics even to be possible, perceptual appearances and the world as a mediating medium must be considered *negligible*: "The negligible is nothing other than what has to be neglected in order to construct physics. . . . What is neglected is always as large as the world" (1968, 32). The neglect is furthered by algebraic formulae and equations that dissolve all real human relationships into "logical relations between man-made symbols" (Arendt 1958a, 264–65, 284; Weil 1968, 37). Once in the world of these symbolic relations, "one inevitably loses sight of the relationship between sign and thing signified," and a form of mechanized petrification sets in (Weil 1973, 93). Hence even as algebraic formulae advance the processes of technology and automation, the formulae themselves proceed automatically and become "impenetrable to the mind," stifling the human capacity to think (ibid.; Arendt 1958a, 5).[10]

Weil's account of automatism, which she calls the triumph of "the blind collectivity" (*une collectivité aveugle*), is written in the turbulent and authoritarian climate of Europe in the early 1930s and replete with images of grinding mechanisms, vast crushing forces, and a domain of control in which "there is no means of stopping the blind trend of the social machine towards an increasing centralization" (1973, 120). Arendt's account of automatism, which she calls the victory of *animal laborans*, is written in the relatively placid but paranoid consumer atmosphere of the United States in the 1950s and appropriates metaphors of the biological life cycle to characterize "the reckless dynamism of a wholly motorized life process" that is inherently futile and worldless (1958a, 132). These temporal and metaphorical differences are more matters of degree than kind, however, given Arendt's references to motors and machines (1958a, 93, 322), and Weil's inclination to view automatism as the pressure of nature and necessity upon human action (1973, 63). What they share is an account of advanced society as a domain of endlessly repetitive, artificially natural cyclicalities.

HOW *HOMO FABER* WORKS

Before the deadly and dehumanizing forces of automatism, Weil and Arendt seek to identify the practical context within which human beings as individuals can actualize freedom as a mode of thought in action and thereby regain a sense of being at home in the world. In doing so, they both reject the modernist strategy—so often associated with the liberal historicist outlook of the nineteenth century—that finds freedom in the domination of nature and necessity. They also reject the romantic reaction of the early twentieth century, with its yearning for submergence within nature and necessity. The metaphysical picture behind both Weil and Arendt's accounts is that of an all-pervading necessity that forms the fabric of the world but wherein freedom, as the *recognition* of necessity, is nevertheless a human possibility (Arendt 1958a, 121; Weil 1973, 85).

Given their shared view of automatism as a process that destroys the experience of work as a causally efficacious relationship between human beings and the world, we might expect that both Weil and Arendt would link freedom in some way to working in the world. In fact, in her magnum opus[11] Weil wholly situates the practical context of freedom in "a civilization in which labour would be sufficiently transformed to exercise fully all the faculties," or in what we might call the *purposeful-performance* of work as self-determination (1973, 104). In *The Human Condition*, Arendt wholly situates the practical context of freedom elsewhere. Even as she pays homage to Weil's *Factory Journal*, which she deems "the only book in the huge literature on the labor question which deals with the problem without prejudice or sentimentality," Arendt entrusts freedom to "men's acting and speaking directly *to* one another," or to what we might call the *theatrical performance* of politics as self-revelation (1958a, 131 [n. 83], 183).[12] These different strategic moves and their implications for the practical context of freedom are directly related to the different ways in which Weil and Arendt think about how *homo faber* works. The practical political inadequacy of Arendtian public realm theory originates here as well. But to see this, we need first to consider how Weil thinks *homo faber* works as *homo methodus*[13] and how Arendt thinks *homo faber* works as *homo fabricatus*.

Homo Methodus

In Weil's theory of action, *work* is a critical concept that designates a normative and emancipatory form of human activity. Loosely following Marx, she uses the term to distinguish both a human form of reproduction and a value sphere within which the human species gains access to reality. Thus, when labor is "sufficiently transformed to exercise fully all the faculties," it forms "the human act *par excellence*" (1973, 104). But Weil also objects that in Marx, "the locus of the process of freedom was transferred from the individual to the species" (McLellan 1990, 72).[14] There is an insistence upon individual subjectivity in Weil's concept of work, and it takes form in the "garden contrast" (Winch 1989, 96) between work, "wherein only efforts exclusively directed by a clear intelligence" determine the expenditure of the working subject's effort, and "blind operations" (*d'opérations aveugles*), where "the mind is weighed down" and movements are beyond control of the intelligence (Weil 1973, 98, 94). Here Weil introduces the idea that if human beings are to escape the humiliation foisted upon them by automatism, they must recover the capacity for methodical thought (*la pensée méthodique*). This is the driving theme of her magnum opus, and the focus of her thought–action account of freedom as well.[15]

For Weil, methodical thinking is a kind of activity in which the subject independently conceives of, coordinates, and carries out physical efforts or undertakings in the external world.[16] Such action is analogous, she argues, to solving properly a problem in arithmetic or geometry. Thus, "a completely free life would be one wherein all real difficulties presented themselves as kinds of problems, wherein all successes were as solutions carried into action" (1973, 86). The analogy to mathematical problem solving posits freedom as the activity of enacting a solution (success) in response to a problem, once identified. When freedom is realized in work, then, it is as a mode of instrumental action in which the subject determines his or her own activity as *homo methodus*, according to his or her own problem-solving initiative. Thus, Weil writes, "the absolutely free man would be he whose every action proceeded from a preliminary judgment concerning the end which he set himself and the sequence of means suitable for attaining this end" (p. 85).

On its face, the analogy Weil constructs between methodical thinking and problem solving has its drawbacks—not the least of which involves its appeal to mathematics (Winch 1989, 96–99).[17] There is another drawback evident here as well, for with its overt appeal to means and ends, Weil's analogy appears to connect freedom with the Weberian notion of purposive-rational (*zweckrational*) action that reduces life activity to specific instrumental properties. But this is not, in fact, the case. Indeed, if we view purposive-rational action as (1) geared toward the production of objects, (2) directed toward the domination of nature, (3) governed by technical models, and (4) driven by "success" in the securing of a goal or an end, then Weil's concept of methodical thinking must be taken as an action type of considerably different cognitive and ethical magnitude.

To see how this is so, we might first return to the problem orientation that Weil advances as the context within which methodical thinking takes place. Unlike concepts of work that take object production as their primary context, Weil's concept places the human appropriation of "means" within a kind of thought–action experience through which the subject perceives the world not as a construction site but as a domain of obstacles, challenges, or perplexities open to possible solution. The emphasis thereby shifts from the making of things to the conscious identification of, and response to, "things thrown forward" or problems (Gk. *pro*-forward + *ballein*-to throw). The kind of work activity Weil has in mind assigns a secondary status to the single end-product of work and primary status to problem identification and the undertaking of a solution within a circumscribed context (1973, 104). Only within the context of responding to challenges, Weil argues, does the subject "dispose[s] of his own capacity for action" and exercise "fully all the faculties" by bringing judgment to bear upon circumstances (p. 85).

Weil also distinguishes the problem orientation that is methodical thinking from the purposive-rational urge to conquer or dominate nature. Here we might attend to the metaphors of high seas and sailing she often uses, in order to grasp how she distances work from an act of violent overcoming—for example, "The intelligence is powerless to get its bearings amid the innumerable eddies formed by wind and water on the high seas; but if we place in the midst of these swirling waters a boat whose sails and rudder are fixed in such and such a manner it is possible to draw up a list of the actions which they can cause it to undergo. All tools are thus, in a more or less perfect way, in the manner of instruments for defining chance events" (1973, 89; see also pp. 88, 91, 101). In this image, nature is both a zone of danger and a domain of possibility, not an inert force to be wrestled under control. The kind of voyaging spirit that Weil has in mind is not that of "the Dutch sea-captain who would go through hell for gain, even though he scorched his sails" (Weber 1958, 57). In the latter case, the boat is a ship, and the ship and its sails are nothing more than expedients for the captain's ruthless pursuit of booty. Weil's sailor is occupied differently. Apparently immune to the daring of adventure and without port or destination, the sailor brings the boat to life simply in the activity of engaging the sea, in a methodical undertaking that risks intelligence, not temerity, against chance. In this image, the tools of work are not directed toward domination, nor are they mere conveyances. They are rather instruments through which the methodical thinker initiates and attempts to maintain a tentative equilibrium within circumstances that challenge and constantly change.

Because the methodical thinker must "continually use craft and strategy, set sails and rudder, transmute the thrust of the wind by means of a series of devices" (Weil 1973, 91), he or she cannot find freedom in a resting place or glory in victorious struggle. Indeed, Weil argues, it matters little whether actions are "crowned with success" (p. 85). Failure can bring unhappiness, but it cannot *humiliate* if a human being resolutely acts "under control of the mind" by filtering an "undefined mass of possible accidents" into a "few clearly defined series" (p. 88). This is also to say that such a person acts in full recognition of the powers of nature; hence Weil appreciatively cites Bacon's dictum, "We cannot command Nature except by obeying her" (p. 107). Only in this tentative equilibrium between body, mind, and universe, where freedom is "near the threshold of the danger of servitude" (Nietzsche 1968, 92) can one locate the ineffable skill of methodical thought. Methodical thinking, it seems, values a particular form of navigation as a mode of acting in the world.[19]

When one has excluded from methodical thinking the urge to objectify, dominate, and secure success, it by no means follows that work is completely purposeless, goalless, meaningless—"a snake biting its own tail" (Nietzsche 1968, 81). Weil's exclusions invite us to comprehend work from a different aspect, but they do not erase purposefulness from the picture. The great stimuli, in fact, are problems that, once identified, arouse thoughtful attention. The more work "throws up such difficulties," Weil argues, "the more the heart is lifted" (1977, 59). The great engagement is in the conscious undertaking of a method or solution, "when wits are exercised, devices tried, obstacles cunningly eliminated" (ibid.). The great release has "nothing to do with welfare, or leisure or security"; it is actualized in the intelligent navigation of challenges and solutions carried into action (Weil 1973, 101). Only in this form of action does the subject challenge what Václev Havel calls "the *diktats* of automatism" (1985, 34) through work that is problem-oriented but not objectifying, masterful but not dominating, and purposeful but not preoccupied by a single "end." Not surprisingly, then, Weil argues that *all* human activities should "imitate the accuracy, rigor, scrupulousness which characterize the performance of work", lest they "sink into the purely arbitrary" and be submerged by forces beyond control (1973, 85). A neighborly affinity thus distinguishes the conceptual relationship she draws between work as methodical thinking and other human pursuits. This *purposeful performance* concept of human action is at the foundation of Weil's account of freedom.

Homo Fabricatus

When Hannah Arendt thinks about how *homo faber* works in the world, she, too, posits a notion of purposeful human performance against the endlessly repetitive, artificially natural processes of automatism. Thus she argues, in a seemingly Weilian spirit, "Whenever men pursue their purposes, tilling the effortless earth, forcing the free-flowing wind into their sails, crossing the ever-rolling waves, they cut across a movement which is purposeless and turning within itself" (1969, 42). But on closer inspection, Arendt's characterization of the sailor's activity bears little resemblance to Weil's example of the boat as an instrument for defining chance events. For Arendt goes on to identify "purposeful human activities" as those which "do violence to nature" (p. 43). In this image, the sailor as seafarer is a violator who is disposed toward command and control of the natural environment, and the ship cutting across the waves is the instrument through which his domination over nature is extended. The image is powerful, but it also points to a more general problem in Arendt's thinking about how *homo faber* works.

In the tradition of German philosophy, and especially under the influence of Martin Heidegger, Arendt conceptualizes work as an emblem of modernity. She also designates work activity not as a material relation between humans, instruments and nature but as a kind of visionary nightmare rooted in "the utopian hope that it may be possible to treat men as one treats other material" (1958a, 188). The theoretical result of this Heideggerian maneuver is Arendt's concept of *homo faber*, in which she finds personified the "typical attitudes" of a (masculinist) modern age: "his instrumentalization of the world, his confidence in tools and in the productivity of the maker of artificial objects; his trust in the all-comprehensive range of the means–end category; his conviction that every issue can be solved and every human motivation reduced to the principle of utility; his sovereignty, which regards everything as material and thinks of the whole of nature as 'an immense fabric from which we can cut out whatever we want . . . '; finally, his matter-of-course identification of fabrication with action" (ibid., 305–06). With this breathtaking indictment, Arendt summons the spirit of heroic capitalism in modernity, in which all things are treated as means, all acts are oriented toward success, and man conducts himself as lord and master, a "destroyer of nature," who rips and cuts his materials "out of the womb of the earth" in relentless pursuit of the acquisition of goods (pp. 139, 153).[20] With the nullity that is victorious capitalism still to come, Arendt conjures up *homo faber* as the last blast of a ruthless epoch that arose "from the great revolution of modernity" (p. 307). But this conjurer's trick has its costs. In order to indict modernity, Arendt must make *homo faber* a powerful symbolic-historical category, and in order to do this, she must conceptualize the human activity of work in a constricted way. On a symbolic level, that is, Arendt's depiction of *homo faber* is a dramatic, expressive triumph, but on a conceptual level her analysis is a series of rigidifying moves that hammer out the normative content of work activity and destroy its emancipatory content altogether.

Arendt's first move is, perhaps, the most devastating for an emancipatory-action concept of work. In this move, the notion of work as a causally efficacious and meaningful equilibrium between human beings and circumstances (of the sort Weil advances) is quickly resolved into nothing more than the devising of instruments and appropriation of tools for the making of things. Arendt's *homo faber* is simply the shell inside which we find that energetic constructor *homo fabricatus* (1958a, 139–44, 305–07; 1969, 215–17). Situated within a world of artifice, the activity of *homo fabricatus* takes on an orientation that is exclusively directed toward the object. Hence Arendt's second move is to theorize fabrication as an act "where a clearly recognizable end . . . determines and organizes everything" (1958a, 156–57; 1969, 215). Under these auspices, the actual process of engaging in fabricating activity is "secondary and derivative"; *homo faber*, Arendt argues, experiences processes "as mere means toward an end" (1958a, 307). The action structure of work, or what happens *in between* "the definite beginning and the predictable end" is thereby reduced to the negligible, while the end is granted existential priority (p. 144). In Arendt's third conceptual move, this already-stunted notion of work as an objectifying means–end orientation now becomes a thoroughly negative formulation as well. The use of tools in the production of objects is secured to "the degradation of all things into means" (1958, 156), "the limitless devaluation of everything given" (1958, 157; 1969, 216), and the "element of violation and violence" (1958, 139). This move to degradation, devaluation, and violence is decisive. With it, Arendt surrenders the ability analytically to grasp the difference between instrumental action as utilitarian objectification and instrumental action as, in Weil's sense, purposeful performance.[21] As a result, she conceptualizes all forms of means–end relations as necessarily and not just contingently connected to acts of violence, manipulation, and control.

Arendt's final move should come as no surprise. In the attitudes of the modern age that she assigns to the means–end activity of *homo faber*, there is no room for genuine interaction oriented toward teamwork or mutual subjectivity. The only company that grows out of workmanship, she argues, "is in the need of the master for assistants or in his wish to educate others in his craft," against which she opposes "the multiheaded subject," or the teamwork of *animal laborans* (1958a, 161–62). *Homo faber* appears only in the "exchange market" and not in "the specifically political forms of being together with others, acting in concert and speaking with each other" (p. 161; Arendt 1969, 217). To appreciate the rather unimaginative and conceptually constricted nature of (both sides of) this Arendtian opposition, we might compare it to an example that Weil offers of purposeful performance as a form of interaction at work: "A team of workers on a production-line under the eye of a foreman is a sorry spectacle, whereas it is a fine sight to see a handful of workmen in the building trade, checked by some difficulty, ponder the problem each for himself, make various suggestions for dealing with it, and then apply unanimously the method conceived by one of them, who may or may not have any official authority over the remainder. At such moments the image of a free community appears almost in its purity" (1973, 101).

This telling example is characteristically Weilian in its subjectivism (and its yearning for absolute purity). Nevertheless, it realistically differentiates and emancipates where Arendt's formulation congeals and confines. The human situation that Weil's purposeful performance account challenges as contingent and alienating (the herd–team of object producers) is one that Arendt deems intrinsic to labor (as teamwork) and to work (as fabrication). Arendt, it seems, can have no equivalent of Weil's problem-oriented, collectively methodical builders. This is not only because she reduces work to an object orientation that prohibits genuine relations with others but also because she reduces teamwork to the routinized performance of motions that prohibits speech and deliberation.

As the scenario of *The Human Condition* unfolds, then, *animal laborans* and *homo faber* emerge in conflict with each other but also allied against freedom and action (1958a, 144–53, 188, 210). Both the automatism of labor and the means–end instrumentality of work are symbolically opposed to, and normatively distinguished from, Arendt's action concept of politics. The symbolic oppositions that Arendt establishes as *labor:work::action* give her public realm theory much of its courageous character. But at the same time, Arendt's inability to conceptualize instrumental action within a richer situational context than *homo fabricatus* leaves her account of politics practically inadequate and vulnerable to what Weil calls "an unconditional surrender to caprice" (1973, 85).

THE THEATRE WHERE FREEDOM COURAGEOUSLY APPEARS

"As long as we believe that we deal with ends and means in the political realm," Hannah Arendt writes, "we shall not be able to prevent anybody's using all means to pursue recognized ends" (1958a, 229). Elsewhere she argues: "If we insist on applying the category of means and ends to action and human relationships, we shall see that everything comes to stand on its head. . . . A bad deed for a good cause instantly makes our common world a little worse" (1953, 597).

The alarm behind these words is neither trivial nor negligible. Out of the hubris of the modern age Arendt detects a conviction that has spawned a myriad of twentieth century horrors: man can know "only what he makes" (1958a, 228). The conviction can be historically located in Plato, "who was the first to design a blueprint for the making of political bodies" (pp. 227). What Plato enacted was a brilliant and devious conceptual maneuver that associated *archein* ("beginning") with "ruling" and eliminated *prattein* ("achieving"), thereby accomplishing a philosophical act of treachery against politics (pp. 188–89, 222–23). The essence of politics became knowing how to rule. Thus, the polis became an object of order and manipulation, the perplexities of action were reduced to "problems of cognition," and the genuine plasticity of acting was hardened into the solidity of work and making. Consequently, Arendt writes, "the most elementary and authentic understanding of human freedom disappeared from political philosophy" (p. 225).

When the conviction that "man is the measure," with its necessary degradation and objectification of all things, donned modern dress and invaded the realm of human affairs, politics became a means to be put to the purposes of particular ideological ends, into which history itself was drawn as consort. The prosaic dimension of a mentality "that forces one to admit that all means, provided that they are efficient, are permissible and justified to pursue something defined as an end" found inherent plausibility in the formulation of capitalistic economy (Arendt 1958a, 229). Its murderous dimension took shape in the Leninist line of argument ("You cannot make a table without killing trees, you cannot make an omelet without breaking eggs, you cannot make a republic without killing people") and reached its most virulent and horrific form in the totalitarian state (Arendt 1969, 139). This ideological orientation toward politics, with its ferreting out patterns in history, distortion of all things into means for the pursuit of allegedly higher ends, violent appeals to new orders and final solutions, and utter contempt for human personhood and individuality, is what Arendt's action concept of politics courageously attempts to overcome. Consequently, Arendtian public realm theory risks a theatrical politics that "lies altogether outside the category of means and ends" against an ideological politics as the pursuit of ends that justify all means (1953, 597; 1958a, 207).

Outside the category of means and ends, Arendt heroically stakes freedom in the condition of "plurality," or the common situation in which human beings reveal their "unique distinctiveness" through acting and speaking together (1958a, 176–77, 202). In the phenomenal space "where I appear to others as others appear to me," Arendtian politics recovers the world that the tremendous cosmos of modern automatism threatens to obliterate. The world is recovered in "the living essence of the person as it shows itself in the flux of action and speech" (Arendt 1958a, 181, 198). Daring to enter the space of appearances, every individual leaves behind the concern with life, and finds in the courage to speak a personal identity and specific uniqueness brought to light. In this condition only, one is not a *what* but a *who*—a name, a physical presence, a living essence, a character-in-voice—"distinguished from any other who is, was or will ever be" (pp. 175; see also pp. 179–81). The "intercourse of the citizens of the polis" is the theater in which mortal actors seize reality as unique and distinct speakers of words and doers of deeds (Arendt 1958a, 180; idem 1969, 222).

Consequently, politics is the space of appearances in which, as Arendt declares, the "inner affinity of speech and action, *their spontaneity and practical purposelessness*" interrupt the "circular movement of biological life" and thwart the drive to calculate a trajectory of human affairs (1958a, 177, n. 1, emphasis mine; 1969, 43). In the name of this "inner affinity" that is freedom, an Arendtian politics bravely defies both the artificial, repetitive, cyclicalities of *animal laborans* and the durable, means–end lineations of *homo faber*. In Arendtian public realm theory, then, the polis is neither a nostalgic yearning nor a perverse Hellenic fixation. It represents metaphorically what "politics" constitutes literally—"a kind of theater where freedom could appear" (1969, 154).

If one of the markers of an emancipatory political theory is its capacity to speak meaningfully about individuals as social and political beings and not just isolated subjects against nature, then Arendt's theatrical performance concept of freedom has some considerable advantages over Weil's purposeful performance concept. Primarily, it carries with it a sure grasp of the emancipatory value of speech as interaction in plurality and a vivid presentation of politics as the "web of human relationships" in which such interaction is actualized through speech (Arendt 1958a, 178). In short, if "politics are wholly instrumental," as M. I. Finley insists (1983, 97), then they can only be instrumental and *meaningful* if they are conceptualized as a *common* human enterprise and against the background of something like plurality in an Arendtian sense. To the extent to which Weil bears allegiance to a post-Cartesian philosophy of consciousness, she can do none of this—neither conceptualize action intersubjectively, nor theorize speech as the activity of mutual recognition geared toward some kind of understanding, nor present politics in the context of genuine commonality. (In fact, Weil does not seem to consider politics a genuine human activity at all. It does not even appear on the list of activities she thinks should imitate work.)[22] Interaction, in Weil's thinking, is at most the cooperation or the coordination of individualistic, disaggregated particulars, like her methodical builders. Speech, in Weil's thinking, seems to be at best a kind of "awkward substitute for sign language"; it is simply the vehicle through which the builders advance the solutions they have pondered in work (Arendt 1958a, 179). Consequently, Arendt's theory of action remains superior to Weil's—not only because it rescues politics within a context of interaction as speech but also because it makes freedom the raison d'être of politics itself (Arendt 1969, 154).

Yet in arriving at this powerful action concept of politics, Arendtian public realm theory also stands in danger of—in Weil's words—sinking into the "purely arbitrary" (1973, 85), for without a dimension of substantive purposefulness that finds positive expression in the vocabulary of "problem," "solution," "means," "end," and "method," Arendt's politics cannot embrace performance as the carrying out or active pursuit of purposes in the very world it strives to vitalize. Politics can encompass performance as public presentation, as in the act of performing a play, a part, or a piece of music: the performing arts "have indeed a strong affinity" with Arendtian politics (Arendt 1969, 154). But without a substantive purpose, Arendt's courageous political performer is constantly in danger of becoming only an actor, "concerned merely with the 'impression' he makes" (Weber 1946, 116). When this happens, the politics of the public realm threatens to become an unconditional surrender to caprice. Moreover, it leaves the theory behind it vulnerable to charges of aestheticism, sentimentalism, and self-defeat. Arendt's public realm theory is dangerously close to this surrender to the arbitrary and the capricious not because it rescues theatrical performance in the face of reductive and routinizing life processes but because it celebrates the "practical purposelessness" of speech and action and the transcendence of "mere productive activity" in politics as it does so (1958a, 177, 180).[23]

By this I do not mean to redeploy the familiar claims that the Arendtian public realm is a talk-shop full of speechifying, "the plaything of an already educated elite," or a kind of game playing "set apart from the real world" (Elster 1983, 98; Schwartz 1989, 30, 41; Udovicki 1983, 56). Arendt in fact anticipates that in a society that celebrates the artifice of *homo faber* and the life of *animal laborans,* her concept of politics (and action) would probably be taken as a narcissistic tribute to "idleness, idle busybodyness and idle talk" (1958a, 208; see also pp. 159, 180). Accordingly, she is careful to assign certain a priori features to words and deeds (i.e., they are "*about* some worldly objective reality") and to action (i.e., it has a "specific content" and a "specific productivity") (1958a, 182, 187, 190–91). These comments obviously failed to innoculate Arendt's public realm theory against charges of irrelevance and elitism, but they indicate that she knew that politics were not simply acts of self-revelation—splendid, courageous, and immortalizing though they may be.

Still, the acknowledgements a theorist makes are one thing, and what the theory can conceptually vindicate is quite another. I am suggesting that Arendt's theory cannot conceptually vindicate either the emancipatory value of purposeful performance or the practical purposefulness of politics.[24] We can trace this conceptual failure in part to Arendt's thinking about how *homo faber* works. Had she initially differentiated between *homo fabricatus* and *homo methodus,* she might have recognized that there is nothing *necessarily* violent, dangerous, or ideological about the category of means and ends in politics. Rather, everything hinges upon the action context within which that mode of thinking takes place. But by assuming that the category of means and ends is necessarily (and not just contingently) afflicted with violent objectification, Arendt has to deny (or at least evade) the normative dimension of the active pursuit of substantive purposes in the space of appearances. Thus, although means–end activity is not excluded from Arendtian politics, it is always and only a negative formulation tied to fabrication.[25] Ultimately, Arendt locates means–end activity in its least negative form in the "prepolitical" domain of legislation (1970, 208–10). In its most negative form, where it slides into something like automatism, it emerges in the "antipolitical" domain of administration (pp. 276–78). Accordingly, Arendtian public realm theory cedes the most important aspects of political activity as everyday life to charismatic founders on the one hand and administrative teams on the other. There must be another way to think about politics.

METHODICAL THINKING IN THE SPACE OF APPEARANCES

Earlier, in considering the means–end category in politics, I suggested that everything hinges upon the action context within which this mode of thinking takes place. I now want to suggest that there is a richer conceptual context—beyond utilitarian objectification, rational capitalist accumulation, and/or Leninism—within which to think about the category of means and ends. Weil offers this alternative in her account of methodical thinking as (1) problem-oriented, (2) directed toward enacting a plan or method (solutions) in response to problems identified, (3) attuned to intelligent mastery (not domination), and (4) purposeful but not driven by a single end or success. Although Weil did not even come close to doing this herself, we might derive from her account of methodical thinking an action concept of politics. Methodical politics is equally opposed to the ideological politics Hannah Arendt deplores, but it is also distinct in important respects from the theatrical politics she defends.

Identifying a problem—or what the philosopher David Wiggins calls "the search for the *best specification* of what would honor or answer to relevant concerns" (1978, 145)—is where methodical politics begins.[26] It continues (to extrapolate from Weil's image of the methodical builders) in the determination of a means–end sequel, or method, directed toward a political aim. It reaches its full realization in the actual undertaking of the plan of action, or method, itself. To read any of these action aspects as falling under technical rules or blueprints (as Arendt tends to do when dealing with means and ends) is to confuse problem solving with object making and something methodical with something ideological. By designating a problem orientation to political activity, methodical politics assigns value to the activity of constantly deploying "knowing and doing" on new situations or on new understandings of old ones. This is neither an ideological exercise in repetition nor the insistent redeployment of the same pattern onto shifting circumstances and events. The problem orientation that defines methodical politics rests upon a recognition of the political domain as a matrix of obstacles where it is impossible to secure an ideological fix or a single focus.

In general, then, methodical politics is best understood from the perspective of "the fisherman battling against wind and waves in his little boat" (Weil 1973, 101) or perhaps as Michael Oakeshott puts it: "In political activity . . . men sail a boundless and bottomless sea; there is neither harbour for shelter nor floor for anchorage, neither starting-place nor appointed destination" (1962, 127).[27] Neither Weil's nor Oakeshott's is the perspective of the Platonist, who values chiefly the modeller who constructs his ship after pre-existing Forms or the pilot–philosopher who steers his craft to port by the light of immutable Forms fixed in a starry night. In both of the Platonic images (where the polis is either an artifact for use or a conveyance to safe harbor), a single and predictable end is already to hand. Neither Weil's nor Oakeshott's images admit any equivalent finality. The same is true of methodical politics, where political phenomena present to citizens—as the high sea presents to the sailor—challenges to be identified, demands to be met, and a context of circumstances to be engaged (without blueprints). Neither the assurance of finality nor the security of certainty attends this worldly activity.

In his adamantly instrumental reading of politics in the ancient world, M.I. Finley makes a similar point and distinguishes between a problem orientation and patterned predictability by remarking upon the "iron compulsion" the Greeks and Romans were under "to be continuously inventive, as new and often unanticipated problems or difficulties arose that had to be resolved without the aid of precedents or models" (1983, 53). With this in mind, we might appreciate methodical politics as a mode of action oriented toward problems and solutions within a context of adventure and unfamiliarity. In this sense, it is compatible with Arendt's emancipatory concept of natality (or "new beginnings") and her appreciation of openness and unpredictability in the realm of human affairs.

There are other neighborly affinities between methodical and theatrical politics as well. Both share a view of political actors as finite and fragile creatures who face an infinite range of possibilities, with only limited powers of control and imagination over the situations in which they are called upon to act. From both a methodical and a theatrical vantage point, this perpetual struggle that is politics, whatever its indeterminacy and flux, acquires meaning only when "knowing what to do and doing it" are united in the same performance (Arendt, 1958a, 223). Freedom, in other words, is realized when Plato's brilliant and devious conceptual maneuver is outwitted by a politics that opposes "the escape from action into rule" and reasserts human self-realization as the unification of thought–action in the world (pp. 223–25). In theatrical politics, however, the actual action content of citizen "knowing and doing" is upstaged by the spectacular appearance of personal identities courageously revealed in the public realm. Thus Plato's maneuver is outwitted in a bounded space where knowing what to do and doing it are disclosed in speech acts and deeds of self-revelation in the company of one's fellow citizens. In contrast, methodical politics doggedly reminds us that purposes themselves are what matter in the end, and that citizen action is as much about obstinately pursuing them as it is about the courage to speak in performance. So, in methodical politics, the Platonic split between knowing and doing is overcome in a kind of boundless navigation that is realized in purposeful acts of collective self-determination.

Spaces of appearances are indispensable in this context, but these spaces are not exactly akin to "islands in a sea or as oases in a desert" (Arendt 1970, 279). The parameters of methodical politics are more fluid than this, set less by identifiable boundaries than by the very activity through which citizens "let realities work upon" them with "inner concentration and calmness" (Weber 1946, 115). In this respect, methodical politics is not a context wherein courage takes eloquent respite from the face of life, danger (the sea, the desert), or death: it is a daily confrontation wherein obstacles or dangers (including the ultimate danger of death) are transformed into problems, problems are rendered amenable to possible action, and action is undertaken with an aim toward solution. Indeed, in these very activities, or what Arendt sometimes pejoratively calls the *in order to*, we might find the perpetuation of what she praises as the *for the sake of which*, or the perpetuation of politics itself (1958a, 154).

To appreciate the emancipatory dimension of this action concept of politics as methodical, we might now briefly return to the problem that Arendt and Weil think most vexes the modern world—the deformation of human beings and human affairs by forces of automatism. This is the complex manipulation of modern life that Havel describes as the situation in which everything "must be cossetted together as firmly as possible, predetermined, regulated and controlled" and "every aberration from the prescribed course of life is treated as error, license and anarchy" (1985, 83). Constructed against this symbolic *animal laborans*, Arendt's space of appearances is the agonistic opposite of the distorted counterfeit reality of automatism. The space of appearances is where individuality and personal identity are snatched from the jaws of automatic processes and recuperated in "the merciless glare" of the public realm (Arendt 1969, 86). Refigured in this fashion, Arendtian citizens counter reductive technological complexes in acts of individual speech revelation that powerfully proclaim, in collective effect, "This is *who* we are!"

A politics in this key does indeed dramatically defy the objectifying processes of modern life—and perhaps even narratively transcends them by delivering up what is necessary for the reification of human remembrance in the "storybook of mankind" (Arendt 1958a, 95). But these are also its limits. For whatever else it involves, Arendtian politics cannot entail the *practical* confrontation of the situation that threatens the human condition most. Within the space of appearances, Arendt's citizens can neither search for the best specification of the problem before them nor, it seems, pursue solutions to the problem once it is identified, for such activities involve "the pursuit of a definite aim which can be set by practical considerations," and that is *homo faber*'s prerogative and so in the province of "fabrication," well outside the space of appearances where means and ends are left behind (pp. 170–71). Consequently, automatism can be conceptualized as a "danger sign" in Arendt's theory, but it cannot be designated as a problem in Arendt's politics, a problem that citizens could cognitively counter and purposefully attempt to resolve or transform (p. 322).

From the perspective of methodical politics, which begins with a problem orientation, automatism can be specified and encountered within the particular spaces or circumstances (schools, universities, hospitals, factories, corporations, prisons, laboratories, houses of finance, the home, public arenas, public agencies) upon which its technological processes intrude. Surely something like this is what Weil has in mind when she calls for "a sequence of mental efforts" in the drawing up of "an inventory of modern civilization" that begins by "refusing to subordinate one's own destiny to the course of history" (1973, 123–24). Freedom is immanent in such moments of cognitive inventory, in the collective citizenwork of "taking stock"—identifying problems and originating methods—and in the shared pursuit of purposes and objectives. This is simply what it means to think and act methodically in spaces of appearances. Nothing less, as Wiggins puts it, "can rescue and preserve civilization from the mounting irrationality of the public province, . . . from Oppression exercised in the name of Management (to borrow Simone Weil's prescient phrase)" (1978, 146).

Perhaps we have now gained some clarity on this picture of methodical politics, but a problem of value remains. Boards can be tools of violence, and so can boats. So the problem, simply stated, involves the ethical constitution of the common context within which methodical thinking takes place. What normative prerequisites are necessary in order to guarantee that methodical politics does not disintegrate after all into the ideological politics that Hannah Arendt identifies as objectifying others and rightly deplores? Against what kind of background can a public realm politics of means and ends, problems and solutions, be secured as methodical and *ethical*? Perhaps this quintessential problem of politics (how to judge rightly the lesser evil, the relatively best, the ends that justify the means) is, in the end, simply too much for Arendtian public realm theory to bear—hence Arendt's ultimate evasion of the category of means and ends in politics altogether. Or perhaps (as some thoughtful scholars argue), Arendt's condition of plurality itself provides the ethical dimension that a truly emancipatory politics—even a politics of means and ends—requires.[28] I cannot answer this problem of value today. In reflecting upon it, I am drawn to the advice that Weber offers on a related problem in science: "If you remain faithful to yourself, you will necessarily come to certain final conclusions that subjectively make sense" (1946, 151). Of course, this probably does not suffice. In any case, I must simply acknowledge that for now we have come to the limits of methodical politics.

THE PLASTIC PEOPLE OF THE UNIVERSE

Even if it could be scrupulously formulated as free and ethical, a methodical politics of problems, plans, and purposes might still come to public realm theory at too great a cost. Ultimately, an Arendtian might argue, it threatens to reimprison human beings in the very existential "solidity" that theatrical politics (with its luminous sense of boundlessness, unpredictability, and irreversibility) is, in the first place, an attempt to overcome. It is true that one of Arendt's greatest accomplishments involves the rescue of politics from what Sheldon Wolin calls its "sublimation" into organization and its "absorption" into nonpolitical institutions and activities (1960, 352–53). The spatial and temporal phenomena that are set forth with atomic-age urgency in Arendt's space of appearances defy the solidity, relentless automatism, and patterned predictabilities that weigh down human existence in the modern world.[29] Humanness is, therefore, regained in the thrills, the highs, and the almost apocalyptic energy of spontaneous word and deed and in new beginnings (natality) that give rise to the totally unexpected.

That is why, when Arendt looks to history, she recaptures the "lost treasure" of action in those moments when politics is redeemed as the sudden, spontaneous release of numbers of people, whose "sheer momentum of acting-together" brings forth the cry for freedom: the American Revolution, the revolutionary councils that appeared in Europe in 1848, the Paris Commune in 1871, the "soviets" in Russia in 1917, the November revolutions in Germany and Austria after World War I, the revolt in Hungary in 1956, and the antiwar and Civil Rights movements in the United States (1958b, 4–5, 26–28). We can now confidently add to this list Charter 77, Worker's Defense Committee, Solidarity, Civic Forum, Public Against Violence, Tiananmen Square, a second Russian revolution, and the truly glorious spectacles of 1989: Warsaw, Prague, Budapest, Berlin. One can only wish that the curator of lost treasures had lived to witness them.

We do disservice to these moments of Arendtian action, however (and hence to politics itself) if we stake acting together solely on "sheer momentum" or the "spontaneity" of rare movements that burst out against the dark backdrop of modernity. To render as truly political only events that are public, spontaneous, and momentous is to underestimate the full complexity (and sometimes the brutality)[30] of human conduct in such events themselves—to see them, as Václav Havel puts it, "from the outside" and perhaps "chiefly from the vantage point of the system and its power structure" (1985, 49). Charter 77 "came as a surprise" and appeared as a "bolt out of the blue", but, as Havel reminds us, it was neither a bolt out of the blue nor the result of a spontaneous political event. Its initial impetus was a small protest against the impending trial of the rock group The Plastic People of the Universe, whose music displeased the communist authorities. The protest began with a campaign planned in detail and with "modest, internal steps" that culminated in the signing of a petition by 70 people (Havel 1990, 130–38). The action group Charter 77 eventually emerged out of the opposition circles that the campaign for The Plastics had informally organized. As a document, the charter took form slowly during the late months of 1976, not in the merciless glare of the public but in what Havel calls "that semi-darkness where things are difficult to chart or analyse" (1985, 49). Its history has as much to do with the laborious organization of meetings, the meticulous crafting of language, the arduous collection of signatories, and the repeated drafting of copies of the original document as with the "explosion" that followed its release in the public realm. Even then, as Havel understands it, the charter was neither a "one-shot manifesto" nor by any means a prepolitical act of legislation but rather a commitment "to participate in ongoing work" (1990, 139).

Like Havel and in the spirit of Arendt's instruction that we must "think what we are doing," I have been thinking about what it means to consider politics as a kind of ongoing, methodical work in the world. I raise the example of Charter 77 not to diminish the beauty of an Arendtian politics of spontaneity but in order to propose a public realm theory that is better able to coordinate political action as purposeful and hence open to a broader range of significance "in the whole way of life" (Weber 1946, 77). In thinking about the same sort of things, Havel warns that the global automatism of technological civilization poses a "planetary challenge" to the position of human beings in the world (1985, 90). If he is right, then those of us who take the project of emancipation seriously must do no less than face the challenge with all the means at our disposal and endeavor, in Simone Weil's words, "to introduce a little play into the cogs of the machine that is grinding us down" (1973, 121). As citizens, in other words, we must think methodically about what is to be done.

Notes

Many thanks to my colleagues Terence Ball and James Farr for their invaluable counsel and criticism on earlier drafts of this article, to Larry Biskowski for instructive discussions, and to my teacher Hanna Pitkin, who guided my studies of Hannah Arendt.

1. There are, of course, significant differences between Arendt and Habermas (see Habermas 1977; see also Benhabib 1986; Bernstein 1983; Honneth 1982; Villa 1992). Here I wish simply to underscore the distinctively modern commitments to action and an egalitarian politics of speech or communication (over and against work and instrumentalism) that Arendt and Habermas share as theorists of the public realm. None of this implies that public realm theory has nothing to gain from numerous forms of contemporary critique, including Marxism, feminist theory, hermeneutics, poststructuralism, and studies of discourse, ethics, and culture.

2. It is important to note, however, that various critics of public realm theory have lodged important objections to both Habermas and Arendt's analyses of social labor and instrumental action. On Habermas, see Agger 1979; Eyerman and Shipway 1981; Giddens 1982; Honneth 1982; Keane 1975; McCarthy 1981; Winfield 1975. On Arendt, see Bakan 1979; Honneth 1982; Jay 1978; Parekh 1979.

3. Assessing Habermas' conceptual strategy is a more difficult proposition that I cannot address here. His early distinction between work and interaction (later reformulated into the dichotomy strategic/communicative action) is more ambiguous than Arendt's. He allows, for instance, that work can be called *praxis*, yet he also insists upon the irreducibility of work and interaction, or *techne* and *praxis* (1973, 158–59). Habermas has criticized Arendt for screening all "strategic" elements out of politics (1977, 16). Yet at the same time he subsumes strategic action, along with instrumental action, within the sphere of "work," or purposive-rational action, and dissociates the latter (which is based on rational decision and the instrumentally efficient implementation of technical knowledge) from communicative action (based on complementarity, reciprocity, and mutual recognition). An extended journey into this Habermasian morass must be deferred. A helpful attempt to sort out, if not resolve, some of the ambiguities can be found in McCarthy 1981, 16–40, and White 1988, 44–47.

4. I refer specifically to the Polish Solidarity movement, under the leadership of Adam Michnik, the great dissident and practical theorist who is now editor-in-chief of *Gazeta Wyborcza*, Poland's largest daily newspaper (personal conversation with Adam Michnik, University of Minnesota, Minneapolis, 1991). For Michnik's respectful view of Habermas, see Michnick and Habermas 1994).

5. Until recently, in fact, Weil's writings have been rather dimly lit in twentieth-century thought, overshadowed by those of her more luminous French colleagues and perhaps by the oddity of her own remarkable life and intractable personality, which have received much attention. Fortunately, sustained studies of her political and social thought have now begun to appear. See, among others, Blum and Seidler 1989; Dietz 1989; Nevin 1991; McLellan 1990; Winch 1989.

6. See Benhabib 1992; d'Entrèves 1989b; Honig 1992; Villa 1992. None of these commentators (although in some lively disagreements with each other about the "discursive," "communicative," "agonistic," and/or "dramaturgical" status of the Arendtian public realm) contest the strategy that conceptually dichotomizes work:interaction or instrumental:communicative. As a result, they collectively promote an Arendtian perspective on politics that is (1) theoretically constrained, because it presumes the inapplicability of instrumental action to an action concept of politics, and (2) practically constrained, because it leans in the direction of an expressivist (rather than a purposeful) orientation toward collective deliberation, citizen action, and democracy. Of course, Arendt's public realm theory is vulnerable to just this sort of expressivist interpretation and (mis)appropriation, and that is part of the problem.

7. Arendt (b. 1906) and Weil (b. 1909) both lived in Paris in the late 1930s. In the increasingly perilous atmosphere for European Jews, both emigrated to New York City (residing, at different times, on Riverside Drive). Weil stayed in the United States for only seven months, leaving for London in 1941. Despite their intellectual affinities and a shared friendship with the philosopher Jean Wahl, they traveled in different circles and apparently never met. There is no evidence to suggest that Weil, who died in 1943, knew anything of Arendt. Arendt (1958a) was familiar with some of Weil's writings. (Arendt's close friend, Anne Mendelssohn Weil, was not related to Simone Weil.)

8. Without question, the threat of totalitarianism, obvious to Weil in the 1930s—and its unprecedented horrors, which were to become the pressure point of much of Arendt's subsequent political thought—also shaped their shared sense

of "the process of world alienation" in the twentieth century (Arendt 1958a, 257). Yet it is also true that both of them understand totalitarianism within the context of technical progress, objectifying processes and dehumanizing institutions that distinguish late modern societies, rather than viewing these developments the other way around, as emanations of totalitarianism. Weil, for example, clearly associates the dissolution of thinking with technical domination and therefore can write, "It is quite unfair to say. . . that fascism annihilates free thought; in reality it is the lack of free thought which makes it possible to impose by force official doctrines entirely devoid of meaning" (1973, 119). A profoundly similar thesis informs Arendt's (1963) notion of the banality of evil in *Eichmann in Jerusalem*.

9. Weil's and Arendt's critiques of algebraic physics are matters of sufficient complexity to warrant study in their own right. Clearly, they share an implicit philosophy of psychology that is critical of attempts to reduce the mind to one or another aspect of a material world that can be fully described by the obvious facts of physics. What such descriptions leave out are phenomena that are *irreducible* to material explanations: thought and consciousness, perception, desires—in short, the intrinsically human world of the senses and appearances. Thus, a phenomenological (or at least perceptual) approach to reality informs both Weil's and Arendt's critiques of contemporary physics, and it is quite obviously carried through in their common admiration for geometry and the science of the Greeks, as well as their respective analyses of the history and social construction of science, the problem of objectivity (or the "Archimedean point"), and the violence that symbolic mathematics exerts upon "common sense" (see Arendt 1958a, 1969; Weil 1968). Thanks to one of her graduate students, Arendt was apparently familiar with Weil's essay "Reflections on Quantum Theory": she mentions it briefly in *The Human Condition* (1958a, 287–88, n. 53).

10. Philosophers refer to this operation as following the "rules of syntax," according to which someone could be trained to do something automatically, using a set of rules, tables, and symbols, without understanding the meaning (or following the "rules of semantics") of the operation as a whole. Although the implications of such syntactical operations have spawned a great deal of philosophical debate in the computer age, Weil obviously anticipated them in 1933 and addressed them within the context of social and political life. Along these same lines, see Arendt's remarks on the logical processes of "the newly invented electronic machines" and their "worldlessness" (1958a, 172) and Havel's discussion of how "a formalized language deprived of semantic contact with reality" replaces reality with pseudoreality (1985, 32–33).

11. This is how Weil characterized "Reflections Concerning the Causes of Liberty and Social Oppression" (Weil 1973), the long essay she wrote in 1934, following a year of hard labor in various French factories located within the "Red belt" outside Paris.

12. Arendt calls the theater "the political art par excellence" because it is "the only art whose sole subject is man in his relationship to others" (1958a, 188; 1969, 154). The allusion to theater calls up the importance of dramatic display in Arendtian politics. It represents the notion of a space wherein human beings appear before each other—each as a uniquely distinct character—and enact a performance that ultimately becomes a part of "the storybook of mankind," a part of theater-history (1958a, 184). In the society of *animal laborans*, we might imagine not only mechanized petrification but also terribly dull theater.

13. With Weil's indebtedness to Greek philosophy in mind, I use the term *methodus* to convey the Greek *met hodos* (*meta*-after + *hodos*-way) and the *Oxford English Dictionary's* "a way of doing anything, esp. according to a defined and regular plan; a mode of procedure" (1.3). *Methodus* does not correspond to *OED* II "a systematic arrangement, order." The latter, as Sheldon Wolin argues, is a usage that originated within the disciplines of logic and philosophy in the sixteenth century (1972, 32–38) and places more emphasis on technique-oriented activity and scientific objectivity than I wish to convey.

14. Rather surprisingly, McLellan also contends that Weil "saw work as inherently servile, . . . as permitting not an embodiment, but a transcendence, of the human condition" (1990, 72). This is quite at odds with the argument in "Reflections Concerning the Causes of Liberty and Social Oppression" (Weil 1973), where she conceives of work as an activity through which the world is disclosed, not transcended. In essays and notebooks written later, however (especially in response to an intensifying mystical turn), Weil emphasizes "the spirituality of work":"Work makes us experience in the most exhausting manner the phenomenon of finality rebounding like a ball. . . . It is when man sees himself as a squirrel turning round and round in a circular cage that, if he does not lie to himself, he is close to salvation" (1952b, 158; see also Dietz 1989, 122–25).

15. I am indebted to Winch's distinction between Weil's "thought–action account of freedom," in which "the point of the action lies in the character of the action itself," and a "desire–satisfaction account of freedom," where "the point of the action depends on its outcome" (1989, 84–85). Arendt also pursues a thought–action account of freedom and criticizes utilitarianism for its "innate incapacity to understand the distinction between utility and meaningfulness" (1958a, 154, 225). As we shall see, however, the dimensions of Arendt's account are significantly different from Weil's.

16. Weil recognizes that conceiving of a method and applying it are two different operations. Even in an emancipatory context, where the agent elaborates and executes the method or "plan of action" at issue, it may be "impossible to combine the examination of these difficulties with the accomplishment of the work" simultaneously (1973, 91–92; Arendt [1958a, 92] makes a similar point). But freedom is only in jeopardy when the elaboration of a method and its execution are decisively divided, and method transfers "its abode from the mind into the matter"—with which the subject works but over which he or she has no mental control (Weil 1973, 92).

17. Weil herself admits, for instance, that "in reality, there is nothing in common between the solution of a problem and the carrying out of an even perfectly methodical piece of work, between the sequence of ideas and the sequence of movements" (1973, 91).

18. Beginning with the seafaring Athenians, sailing becomes a commonplace reference in the discourse and rhetoric of Western political thought. The focus, however, is almost always on a ship (not a boat), which serves as an analogue for the city, or the constitution of the state. As Martha Nussbaum notes, "The city–ship, in the tradition of the image, is something safely water-tight, a barrier against imminent external dangers" (1986, 59). In Weil's image (and in another I shall consider later), the focus is on the action context of sailing and hence upon the sheer complexity of a practical task—in which the boat is not a ship, a barrier against imminent dangers, or a locus of stability in roily waters but rather the means through which dangers are encountered, with a greater or lesser measure of success.

19. In an age of electronic signals, automatic pilots, and computerized navigational instruments, this metaphor, too, loses its meaning. For an instructive parallel to a Weilian perspective on methodical thinking as a human form of navigation with mere "boards" to hand, see William Finnegan's fine essay concerning the legendary surfer "Doc" Mark Renneker of San Francisco and the "ineffable skill" of "wave judgment" in surfboarding the pounding waters of Ocean Beach off Sloat Boulevard (1992b, 46).

20. Feminist critiques of Arendt's political thought have generally overlooked her gendering of the *vita activa*, where we find the oppositional alliance between *animal laborans* (gendered female), and *homo faber* (gendered male) set against the public realm and the condition of plurality. Under these terms, Arendtian politics may be interpreted as (1) an overcoming of the gendered alliance rooted in being and subjectivity through the actualization of doing and interaction and (2) a destabilization of the symbolic order of feminine/mascu-

line through the proliferation of distinctive, individual voices that are not *whats* (i.e. representations of class, race, gender) but *whos* (uniquely distinct personalities). There are, I think, some potentially radical implications here for current feminist discussions concerning the politics of identity.

21. Arendt seems to grasp this differentiation when she writes, "The issue at stake is, of course, not instrumentality, the use of means to achieve an end, . . . but rather the generalization of the fabrication experience [as] the ultimate standard[s] for life and the world of men" (1958a, 157). But she proceeds to characterize instrumentality as "the experience of ends and means, as it is present in fabrication" (ibid.). So although she initially differentiates instrumentality in work from instrumentality as a generalized phenomenon, she nevertheless reverts to a concept of instrumentality that is wholly informed by her (negative) concept of work.

22. Winch does not overstate when he argues that Weil's theory of action views "the individual and his or her thinking in uncompromisingly individualistic terms" (1989, 99). Weil argues, for example, "Several human minds cannot become united in one collective mind, and the expressions 'collective soul', 'collective thought', so commonly employed nowadays, are altogether devoid of meaning" (1973, 82). At a later stage in her work, when she turns more toward ethos, habituation, and the "need for roots" in order to formulate the content of free and ethical life, Weil's negative view of collectivities relaxes (Weil 1952a). But to say that she arrives at a concept of emancipatory dialogic interaction would be a gross exaggeration. For a discussion of Weil's antipolitics, see Dietz 1989 and Ignatieff 1990.

23. On a similar point, see Knauer, who also argues that "Arendt's account of the *vita activa* needs reformulation as a theory of democratic praxis" (1985, 187–90). Knauer's useful reformulation of praxis posits a dialectical relation among *labor:work:action* that, he argues, Arendt herself recognized but did not adequately develop. Thus "the task is to illuminate the ways in which beings gathered in political community could choose to live their lives together as workers and laborers . . . so as to nurture political community" (p. 190). By infusing an action dimension into labor and work, Knauer humanizes Arendt's categories and translocates action (politics) as "political community." The problem with this approach, however, is that it collapses action into labor and work and then calcifies Arendt's concept of politics as a communitarian normative ideal. It thereby evades precisely what needs to be examined, namely, Arendt's concept of politics as a concrete, practical *activity*.

24. In his thoughtful essay, Lawrence Biskowski argues that there is substantive purpose in Arendt's politics, which is "inherently connected to care for the world, not only for what the world thinks . . . but for what the world will be like in the wake of one's acting" (1993, 880). Thus we can find substantive ethical content for Arendtian politics in the doctrine of *amor mundi* (p. 878). In speech and action, citizens keep alive the space of appearances itself. I agree with Biskowski's claim that the concept of "world" that informs Arendt's understanding of politics carries substantive ethical content. What I am interested in here, however, is the substantive *cognitive* content of Arendtian politics. Since Arendt identifies cognition as "the pursuit of an aim which can be set by practical considerations" and aligns it with *homo faber*, fabrication, and scientific processes (1958a, 170–71), I think we have to conclude that cognition plays a very small role in Arendtian politics. Despite its ethical purpose, then, I want to maintain that Arendt's politics is inadequate as a cognitive purposeful enterprise.

25. As Knauer (1985) and d'Entrèves (1989b) correctly note in criticizing Jay (1978), Arendt does not exclude instrumentalism from politics. But my point is that instrumentality is always formulated negatively. What Arendt and her commentators seem to share is a congealed concept of instrumental action that cannot distinguish instrumentality as utilitarian objectification from instrumentality as purposeful performance. This is clear in Knauer's rhetorical query, "Why should [Arendt] emphasize the instrumental aspect of all politics when her aim was to overcome its instrumentalization and trivialization?" (1980, 733). Also see d'Entrèves's observations that action is *constrained and hampered by* instrumental concerns that can never be fully "eliminated" and that politics is emancipatory only when it is able to "transcend mere instrumental concerns" (1989b, 333–35, emphasis mine).

26. Along these lines, Wiggins seeks to recover a dimension of deliberation over ends in Aristotelian practical reasoning. Aristotle, he argues, "has been incorrectly interpreted or translated . . . as saying that deliberation is not of ends. He is always prepared to describe practical reason . . . in its concern with the question what objectives a man *is* to form (in general, of here and now), and what particular concerns he is to put first, in the light of how the Good appears to him" (1978, 150). With this argument in mind, we might understand Weilian methodical thinking (despite her odd dislike of Aristotle) as fully in keeping with some aspects of Aristotelian *phronesis*, although elaborating the linkages is beyond my scope here.

27. I hasten to add that although I find this metaphor for political activity appealing, I do not share Oakeshott's commitment to a "tradition of behavior" as the locus for political life. The notion rests uneasily with the technicization of modern life that has, with tragic consequences, forged a tradition of its own and supplanted precisely those traditions that Oakeshott wants to recover. The best critique of Oakeshott's politics remains Pitkin 1974.

28. There is a wide array of views concerning the ethical dimension of Arendt's politics. Knauer (1980), Hinchman (1984), Issac (1989), and Biskowski (1993)—in various ways—make strong cases for locating an ethical commitment to human dignity, mutuality, and civility in Arendt's condition of plurality and concept of world. Kateb (1984), Jay (1978), and Villa (1992)—in various ways—make strong cases against it. I shall not attempt a consideration of this debate here. Nor is this the place to undertake the philosophical task of justifying the reconciliation of instrumental action with dialogic interaction. For a general discussion (especially as the argument relates to Habermas), see Rasmussen 1990.

29. These repetitious and relentless patterns are also the domain in which "questionnaires and motivation research" and other "gadgets in the arsenal of the social sciences" operate (Arendt 1958b, 8). Arendt's phenomenological rescue of action must, therefore, be understood as a form of resistance against a world in which the theories of behaviorism "could actually become true" (1958a, 322).

30. In a recent symposium on intellectuals and social change in Central and Eastern Europe, Doris Lessing noted that "revolutionary politics, the house committees, the vigilante slogans, are intoxicating drugs" (1992, 727). She also recalled one European commentator's remark about "demonstrations that seem to have little point . . . from the point of view of actually achieving something. . . . The ends are not the point. The means are the point" (ibid.) These remarks serve to remind us that the politics of revolutionary, theatrical spontaneity are as potentially susceptible to meaninglessness and excess (including murderous excess) as the politics of "ends" and ideology that Arendt powerfully condemns. Indeed, these are often elements of the same complex of experience. Compare the violence of Bucharest's revolutionary politics in 1989, for example, to the velvet revolutionary politics of Prague, or even Berlin.

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PATRICIDE AND THE PLOT OF *THE PRINCE*: CESARE BORGIA AND MACHIAVELLI'S ITALY

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An understanding of Machiavelli's assessment of Cesare Borgia in The Prince *is essential for interpreting his view of politics, but the ambiguity of that assessment has led to vastly different conclusions about Machiavelli's political teaching and Cesare's significance. We approach Machiavelli's ultimate intentions through a consideration of his more immediate concern for Italy. Machiavelli's great interest in Cesare and his criticism of this potential hero stem from the historical context of an Italy divided due to the Church. Cesare possessed—yet squandered—an opportunity to rid Italy of the evils plaguing it by killing his father, Pope Alexander, and by eliminating the College of Cardinals. Machiavelli's suggested denouement to the plot of* The Prince *is an assault on the ecclesiastical power. He invites his reader to contemplate the vulnerability of the Church and to act where Cesare and others shrank. Machiavelli ultimately counsels us to break our reliance on God or fortune and thus create the conditions for a reinvigorated civil life.*

Machiavelli confronts his readers in *The Prince* with an ambiguous portrait of Cesare Borgia. This ambitious son of Pope Alexander VI—this ruthless new prince of the Romagna—indisputably occupies a great deal of Machiavelli's attention in the work. Indeed, Machiavelli offers Cesare as an instructive model: "I do not know what better teaching I could give to a new prince than the example of his actions" (1985, 27).[1] Ultimately, however, Machiavelli's assessment of Cesare's achievement is mixed. Despite his outstanding *virtù*, in the end Cesare failed in his enterprise due to "an extraordinary and extreme malignity of fortune": surprised by the death of his father and his own illness, Cesare made an unfortunate choice in supporting the election of Julius II, the future warrior pope (pp. 32–33).

The prominent yet uncertain role of Cesare in *The Prince* is at the center of debates over Machiavelli's political theory. More than one reader of *The Prince* has been led by the relish with which its author relates the murderous deeds of the Borgias to conclude that Cesare is Machiavelli's hero, simply. "Machiavelli was full of his idol, Duke Valentino," declares Montesquieu (*Spirit of the Laws* 29.19). An actual prince eager to prove his humane credentials, Frederick of Prussia, indicts Machiavelli by saying Cesare is "the model on which the author forms his prince" and his "hero" (1981, 58; see also A. Gilbert 1938, 42). This conclusion requires revision in light of Machiavelli's own criticism of his exemplar (Sasso 1966, chap. 14). Some interpreters have therefore argued that Cesare, whatever his shortcomings, is Machiavelli's idealized "man of *virtù*" (Hulliung 1983, 192) or the model for his own method of statecraft (e.g., Butterfield 1962, 97–98, see also Skinner 1981, 8–12, 33–34). Conversely, others have seen Machiavelli's attention to Cesare as evidence that *The Prince* is a satire. "The choice of his execrable hero is in itself enough to make manifest his hidden intention," Rousseau states, claiming that "*The Prince* is the book of republicans" (*Social Contract* 3.6 and n.; see also Mattingly 1958, 487–91). Cesare's place in *The Prince* thus raises questions about Machiavelli's intentions in that work and the relationship of his handbook for princes to his apparently more republican writings, especially the *Discourses*.

As political scientists or historians of political thought, we are ultimately interested in Machiavelli because of the general bearing of his thought and its influence, including his status as perhaps the first truly "modern" political thinker.[2] Nonetheless, Machiavelli did write in the context of sixteenth century Italy. Just as it is a mistake to read *The Prince* as essentially historically bound, so too is it an error to interpret it without reference to Machiavelli's surroundings and his more immediate intentions. We approach Machiavelli's ultimate intentions by concentrating on his more immediate ones in *The Prince*: his concern for an Italy overrun by "barbarians" and kept divided due to the papacy.

We argue that Machiavelli's concern with the Church's ruinous influence on Italy is the source of both his attention to Cesare and his criticism of this potential hero. Although we differ with Rousseau on the issue of whether Machiavelli's interest in Cesare is sincere, we agree with him that the Church is Machiavelli's target in *The Prince*: "The court of Rome has severely forbidden his book. I can well believe it; it is the court that he most clearly depicts" (*Social Contract* 3.6, n.).[3] Because Cesare was perfectly situated to eliminate the power of the Church, he possessed—yet squandered—an opportunity to rid Italy of the evils plaguing it and thus to realize the grand achievement called for in the concluding chapter of *The Prince*. By following the development of the plot of *The Prince* and its protagonist, we uncover Machiavelli's advice for remedying Italy's ills. Like his play

Mandragola (a comedy of conspiracy), *The Prince* has a plot.

Unfolding in a chilling drama, Machiavelli's plot involves the conquest of the ecclesiastical principality of Rome, a special type of state to which he devotes a chapter of *The Prince.* The court of Rome has a peculiar mode of rule where the pope's authority is derived from the cardinals and that of the cardinals from the pope. The complete acquisition of this state would require the elimination of both the pope and the College of Cardinals. Machiavelli intimates that Cesare could have done just that: had Cesare acted more prudently, "he could have kept *anyone* from being pope (*poteva tenere che uno non fussi papa*)" (p. 33 [Machiavelli 1971, 268]; emphasis added). Cesare, however, failed to do this. To employ Machiavellian terminology from a similar context, Cesare was bad, but he failed because he was not altogether bad (see *Discourses* 1.27). Machiavelli approves with studied coolness the crimes the Duke committed in his ascent to power. We argue that for Machiavelli, Cesare's lapse was a sin of omission: he failed to commit a simultaneous homicide and patricide. Machiavelli's suggested dénouement to the plot of *The Prince* represents an assault on the power of the Church and of Christianity generally. Machiavelli thereby inspires one to contemplate the vulnerability of an inimical ecclesiastical power. In countenancing the most extreme measures to eradicate it, he counsels someone who possesses both the virtue and the fortune to act where Cesare and others shrank—whether that person be the immediate addressee of *The Prince,* Lorenzo Medici, the nephew of the pope at that time, or the ultimate audience that Machiavelli indicates—"whoever understands it" (p. 61).

After briefly discussing the historical context to which Machiavelli's immediate intentions in *The Prince* are a response, we follow the plot of *The Prince* through chapter 11, "Of Ecclesiastical Principalities."[4] Our solution to the puzzle of Machiavelli's use of Cesare Borgia not only reveals the coherence of a work many readers find disjointed at first appearance (e.g., Allen 1960, 452; see also Skinner 1981, 23) and ascribes a significance to passages usually deemed insignificant, but also links the darkest, most sinister Machiavellian intent with his concern for Italy. Finally, we address how our interpretation speaks to the vexing problem of the relationship of Machiavelli's republicanism and patriotism to his role as an advisor to princes.

THE PRINCE AND MACHIAVELLI'S ITALY

The historical context in which Machiavelli wrote undoubtedly influenced his thought, but questions abound as to the manner in which and the extent to which it did so. It can be said with certainty that historical context dictated the way in which he presents his thought and provided many of the examples he uses to expound it, notably that of Cesare Borgia. In order to understand Machiavelli's more immediate concerns in *The Prince* and thereby to comprehend his ultimate teaching, knowledge of Machiavelli's Italy is essential. Machiavelli is concerned with Italy's divisions and vulnerability. A number of scholars have sought to uncover Machiavelli's remedy for Italy's ills (or at least his native Florence's), sometimes with attention to how Cesare Borgia's enterprise—the creation of a large northern Italian state with the aid of his father, the pope—is in some way his model for that remedy. The interpretations offered by these scholars are, however, inadequate because they are not radical enough. Because they do not reckon with what Machiavelli indicates is the pervasive cause of Italy's woes—the papacy and its policy—these interpretations do not grasp the extremity of the remedy he prescribes. By examining these interpretations and then turning to Machiavelli's treatment of the papacy, we can establish the magnitude of the problem Machiavelli apprehends and make more plausible the extreme remedy we argue he contemplates.

Italy and Machiavelli's *Prince*

Numerous scholars have approached Machiavelli from a historical perspective, but in steering clear of lamentable ignorance of Machiavelli's Italy they have sometimes underestimated their author. Mary Dietz has done a great service in her own attempt to solve the problem of *The Prince* by reading Machiavelli with his historical situation in mind while at the same time redirecting our attention to the deceptiveness of the theorist of deception. Dietz argues that Machiavelli's work is itself a work of deception, a trap into which he intends Lorenzo Medici to fall: "Machiavelli devises a plot, a series of moves that, if followed, will lead Lorenzo to disaster" and Florence to a renewed embrace of its republican institutions (1986, 781). Dietz's application of Machiavelli's praise of the tactic of fraud to his own writing is well founded. For example, in a letter to Guicciardini of 17 May 1521, quoted by Dietz, Machiavelli declares, "For a long time I have not said what I believed, nor do I ever believe what I say, and if indeed sometimes I do happen to tell the truth, I hide it among so many lies that it is hard to find" (Machiavelli [1989, 2:973]). No longer able to play the lion, he adopts the nature of the fox. Machiavelli is indeed "outspoken, irrepressible, and fearless," as Hulliung insists (1983, 28), but his aims in *The Prince* themselves require subterfuge.

Although Dietz suspects something sinister, she identifies the wrong plot. To begin with, the scope of her investigation is too limited. Granting even that it was Machiavelli's intention to mislead the Medici to their downfall, a reconstituted Florentine republic would still have been vulnerable to the dealings of its neighbors. The republic for which Machiavelli had served as a loyal envoy had, after all, fallen in 1512 as a result of the French defeat at the hands of the Holy League, led by Pope Julius II. The viability of the republic was therefore dependent not only on the organization of Florence but also on that of Italy. Our consideration of *The Prince* reveals that Machiavelli had precisely this problem in mind. Moreover, Dietz takes no account of Machiavelli's offer of Cesare Borgia as a model to be imitated. Surely looking into this mirror of princes would not reveal the vulnerable prince that Dietz suggests would result from Machiavelli's advice.

Attempts have been made to untangle the puzzle of *The Prince* with reference to its Italian context and the model of Cesare Borgia. Most notably, Hans Baron argues that Machiavelli wrote his plea in the concluding chapter of *The Prince* to "redeem" Italy in reaction to "a unique *occasione*" that offered itself to the Medici through their simultaneous hold on Florence and the papacy. Baron suggests that the request is made for the Medici to expel the barbarians "by founding, in accord with the rules established in the book, a strong new state modeled on the one built a decade earlier by Cesare Borgia as a power nucleus on the borders of north and central Italy—a historic enterprise which only misfortune had prevented from becoming the starting point for Italy's liberation" (1991, 85). Whitfield makes a similar argument and concludes that Cesare Borgia "represents the only historical parallel, the proof of how much one can build upon the favour of a pope" (1969, 28; see also Whitfield 1965, 62–64).

Baron and Whitfield mistake the *occasione* that Cesare's example reveals. The client state that Alexander VI was carving out through Cesare was an insufficient remedy for the illness diagnosed by Machiavelli. First, because of the brevity of the average pope's reign and the alternating creation of pontiffs from competing factions—points Machiavelli emphasizes in *The Prince* with reference to Cesare (pp. 32, 46)—any alliance of the papacy and a northern buffer state was certain to be ephemeral. As the head of the papal forces, Giuliano Medici was in the same position as Cesare, but he would be at the mercy of his uncle's successor in the same way that Cesare relied on the bad faith of his father's successor. Second, Baron and Whitfield do not take account of Machiavelli's argument for the ruinous effect of the papacy and his indictment of Christianity itself. Baron does recognize this second problem, but his solution is to read the chapters in the *Discourses* that indict Christianity as later interpolations, just as he seeks to resolve the question of the relationship of the fervent patriotism of chapter 26 of *The Prince* to the cold technique of the rest of the work by ascribing to the last chapter a separate and later composition (1991, 101–2; see Baron 1954). Like Baron and Whitfield, Strauss attends to Machiavelli's concern for Italy and seems to assume that he had in mind the creation of a northern Italian state (1958, 80–81). Strauss does recognize the problem of the Church and Christianity more generally for Machiavelli, although he does not combine the two concerns in the way we suggest. Machiavelli ultimately does not urge Giuliano or Lorenzo Medici, in league with Pope Leo, to imitate Cesare Borgia in the manner Baron or Whitfield suggest. We shall show that Machiavelli's concern with the Church in connection with Italy's woes is present from the beginning of *The Prince* and unifies its development.

Machiavelli's Treatment of the Papacy

The most important aspect of the historical context of *The Prince* for understanding Machiavelli's more immediate intentions in *The Prince* (as well as his ultimate aims) is his view of the influence of the Church on Italy and the problem of Christianity more generally. In *The Prince*, Machiavelli writes that Italy has been "subjected to barbarous cruelties and insults" of the French and Spanish armies and the mercenary forces of the Swiss and Germans (pp. 102, 104–5). In the *Discourses*, as well as in the *Florentine Histories*, he blames the papacy for the political division of Italy, the very problem he laments at the end of *The Prince*:

> The Church has kept and still keeps this region divided. . . . The reason why Italy is not in that same condition and why she too does not have one republic or one prince to govern her is the Church alone; because, though she has dwelt there and possessed temporal power, she has not been so strong or of such ability that she could grasp sole authority [*occupare la tirannide*] in Italy and make herself ruler of the country. Yet on the other hand she has not been so weak that, when she feared to lose dominion over her temporal possessions, she could not summon a powerful man to defend her against anyone who in Italy had become too powerful. (1.12 [Machiavelli, 1971, 96]; see also *Florentine Histories* 1.9)

Machiavelli discusses the temporal power of the Church in *The Prince* at several points. He explains that Louis XII of France erred "by giving aid to Pope Alexander so that the pope might seize the Romagna" and by failing to realize that he was thus "making the Church great by adding so much temporal greatness to the spiritual one that gives it so much authority" (p. 14). Later in the same work, Machiavelli observes of the Church that "before Alexander, the Italian powers, and not only those that are called powers but every baron and lord, even the least, held her in low esteem in temporal affairs" (p. 45). Alexander increased the temporal power of the Church through his son, Cesare. Machiavelli might appear to commend such a strategy when he points to the powerful pontificate found by the Medician pope, Leo X, the uncle of the immediate addressee of *The Prince* (pp. 47, 102; but cf. *Discourses* 2.22).

Machiavelli nevertheless reveals that even a strong Church cannot be the solution to Italy's problems because of its reliance on mercenary arms. He makes this point in *The Prince* immediately following his treatment of ecclesiastical principalities, stating that "the present ruin of Italy is caused by nothing other than its having relied for a period of many years on mercenary arms. . . . And he who said that our sins were the cause [of the French invasion of Italy] spoke the truth. But the sins were surely not those he believed, but the one I have told of" (p. 49).[5] The Church is primarily responsible for this condition: "Since Italy had almost fallen into the hands of the Church and a few republics, and since the priests and the other citizens did not have knowledge of arms, they began to hire foreigners" (p. 52). Citizens can be armed, and Machiavelli dedicated himself to that end in Florence and throughout his writings, but the clergy is always unarmed and at the mercy of others (see p. 81).

Machiavelli expands on the Christian clergy in connection with the political corruption of the moderns in his *Discourses*. When he refers to those who administer to the pope as a "court" (1.12), he establishes that the members of the clergy constitute a type of noble class. Later in the same work he speaks of a type of gentleman (*gentiluomo*) particularly inimical to "any political life," those "who without working [*oziosi*] live in luxury on the returns from their landed possessions," and points his reference when he lists the Papal States (*Terra di Roma*) as one place where such gentlemen can be found in abundance.[6] He ominously reports that the Germans kill these gentlemen when they find them (1.55 [Machiavelli 1971, 138]). Machiavelli challenges anyone who may disagree with his analysis of the responsibility of the "Roman court" for Italy's ills to send this court to Switzerland. He concludes in advance of the experiment that such largess on the part of the Italians would in a very short time create similar disorders even among the Swiss, who, of all modern peoples, live in a manner most akin to the ancients (1.12). Christianity has disarmed a noble class, thereby rendering it useless to life by making it dependent on other arms.

In the *Discourses*, Machiavelli states that "we Italians" have as "our first debt to the Church and to the priests that we have become without religion and wicked" (1.12, see also 1.27). Some readers have argued that this passage is informed by Machiavelli's firm foundation in the Christian faith and concern that the clergy be reformed (e.g., Grazia 1989, 89–90; cf. Parel 1992, 62). Nevertheless, Machiavelli indicts not just the Church for Italy's woes but Christianity for the ills of modern states generally. The Christian religion "has made the world weak and turned it over as prey to wicked men, who can in security control it, since the generality of men, in order to go to Heaven, think more about enduring their injuries than about avenging them" (*Discourses* 2.2). Machiavelli holds out as his utmost standard the exaltation of his earthly homeland (see Machiavelli to Vettori, 16 April 1527 [1989, 3:1010]). A Christianity reformed to his specifications would cease to be recognizably Christian (see Berlin 1980, esp. 46–50; Hulliung 1983, 67). Machiavelli's view of Christianity itself strengthens his indictment of the papacy and precludes a strong Church from being the remedy for Italy's condition.

Machiavelli's discussion in the *Discourses* of the effects of Christianity refers to "modern" peoples generally. He indicates the pervasive influence of Christianity on modern times in *The Prince* when he relates his conversation with Rouen: "For when the cardinal of Rouen said to me that the Italians do not understand war, I replied to him that the French do not understand the state, because if they understood they would not have let the Church come to such greatness" (pp. 15–16). Louis's fault was his faith: (1) the "faith" Louis might be said to have owed Alexander "for dissolving his marriage and for the [cardinal's] hat of Rouen," faith Machiavelli explicitly urges Louis to break by referring the reader to his discussion of faith later in the work (pp. 15–16 and chap. 18, esp. p. 70); and (2) his faith more generally, which Machiavelli implies we should break. The power of Christianity over the modern mind is pervasive, and Machiavelli must make us see the religion as both inimical and vulnerable if we are to break our faith with it and adopt his "new orders" (see pp. 23–24, 61).

Healthy politics in Italy and modern times more generally would require as a first step the destruction of the power of the Church, if not its total elimination. Machiavelli does consider the possibility of the murder of a pope and the destruction of the "Roman court." In the *Discourses* he reports that when Pope Julius II undertook to expel the tyrants who had seized the land of the Church, he entered Perugia to remove Giovampagolo Baglioni. Machiavelli first comments on the rashness of Julius (see 1985, 100–101), who entered the city entirely unarmed while his enemy maintained troops there. But the tyrant Baglioni receives Machiavelli's harshest criticism. Baglioni's "cowardice" permitted the unarmed pope to leave Perugia with his prey. Machiavelli complains: "All the sagacious [*prudenti*] men with the Pope . . . could not reckon whence it came that [Baglioni] did not, to his everlasting fame, at one stroke [*ad un tratto*] put down his enemy and enrich himself with booty, since with the Pope were all the cardinals with all their precious things." Machiavelli is indeed shocked at Baglioni's uncharacteristic omission of a sin from conscience: "So Giovampagolo, who did not mind being incestuous and an open parricide, could not or, to put it better, did not dare, when he had a perfect opportunity [*occasione*] for it, do a deed for which everybody would have admired his courage and for which he would have left an everlasting remembrance [*perpetua fama*] of himself. . . . And he would have done a thing the greatness of which would have transcended every infamy, every peril that could have resulted from it" (*Discourses* 1.27 [Machiavelli 1971, 109–10]; see also Machiavelli 1985, 54).[7]

The greatness of the reward that would accrue from this terrible deed—the deed from which even Baglioni shrunk—suggests the significance of the elimination of the papacy for Machiavelli. Yet his characterization of Baglioni's potential reward does not appear to accord with an earlier remark in the *Discourses*. Whereas "heads and organizers [*ordinatore*] of religions" are "most famous" (*laudati*) of all human beings, "those men are infamous and detestable who have been destroyers of religions, squanderers of kingdoms and republics, enemies of virtue" (1.10 [Machiavelli 1971, 91]). Baglioni's "cowardice" would be indictable, but surely the murderer of a pope and his entourage would seem to merit "eternal infamy" rather than "perpetual fame"—unless, of course, the act not only destroys but also founds. Machiavelli suggests just this conclusion in his *Florentine Histories* when he relates Stefano Porcari's ambition: animated by a desire for glory, the Roman Porcari "judged he could do nothing else than to try to see if he could take his fatherland from the hands of prelates and restore it to its ancient way of life, hoping by this, should he succeed, to be called the new founder and second father of [Rome]" (6.29). Porcari's "mode" entailed killing the pope and calling the disaffected people to arms against the Church. Porcari failed in his conspiracy (see *Discourses* 3.6), and Machiavelli comments, "The intentions of this man could be praised by anyone, but his judgment will always be blamed by everyone because such undertakings, if there is some shadow of glory in thinking of them, have almost always very certain loss in their execution" (*Florentine Histories* 6.29). Machiavelli blames only Porcari's failure to acquire, not his aim. "Machiavelli more than hinted that the time had come for a political takeover of the church," Hulliung comments; "Where Stefano Porcari and Cesare Borgia had failed, others could succeed" (1983, 217). Hulliung thus intimates the solution, although he fails to indicate the means to that end as it comes to light in *The Prince*.

The person who removes the religious blight from Italy would provide the possibility for the growth of healthier political institutions. As Machiavelli repeatedly emphasizes in many contexts, the very act of founding or reforming requires horrifying deeds. Most prominently, Machiavelli's first example of a prince by virtue, Moses, had to commit terrible acts and, even if Machiavelli only implies it, eliminate a former religion (*Discourses* 2.5, 3.30; see, esp., Exodus 32:27–28). Machiavelli's other instances of the greatest exemplars are similarly guilty. Romulus, for example, committed fratricide and consented to the death of his colleague, Titus Tatius. Romulus must be forgiven, however, because his object was to found *un vivere civile* (*Discourses* 1.9 [Machiavelli 1971, 90]; see also 1.18, 3.1).[8] Cruelties "well used" are necessary to establish political order and vibrancy. Perhaps if Baglioni had had a similar end in view, his deed of papacide, like that of Romulus's fratricide (or Theseus' possible patricide),[9] would have earned him fame rather than infamy. Borgia had the same opportunity as did Baglioni and Porcari; if he had seized it, Machiavelli would certainly rank him beside Romulus and the others as "one of the most excellent of princes."

THE PLOT OF *THE PRINCE*

Machiavelli discusses his greatest exemplars—new princes who acquire through their "own arms and virtue"—in chapter 6 of *The Prince*. Those who acquire through "others' arms and fortune"—the theme of chapter 7—pale in comparison. Yet Machiavelli offers these princes, notably Cesare Borgia, as examples to be imitated. Unlike the wholly or partially mythical exemplars of chapter 6, Cesare was of flesh and blood. Hulliung suggests that Cesare is "an abstraction Machiavelli created by taking elements from both Greek and Roman thought and compounding them into a creation all his own" (1983, 192). We insist upon the historical Cesare as the key to grasping Machiavelli's more immediate intentions in *The Prince*. Through Cesare's example Machiavelli instructs a prince not only not to rely on the arms and fortune of others but also how to solve Italy's political problem.

As we have shown, the politics of Italy in particular and of modern times in general was decisively affected by the Church. In order to understand Machiavelli's remedy for Italy, one has to read *The Prince* from the very beginning with the discussion of ecclesiastical principalities in chapter 11 in mind. Wolin is therefore incorrect when he states that "Machiavelli contended that ecclesiastical governments were irrelevant to the proper concerns of the new science" he was developing and that they were "not politic enough to warrant the attention of political thought" (1960, 198–9). The chapter on ecclesiastical principalities does indeed appear to be almost an afterthought, since the ecclesiastical state of Rome does not fit into Machiavelli's initial categorization at the outset of *The Prince* and is not included there. This appearance is not inadvertent, however, and the anomalous position of that discussion is actually the culmination of an argument that develops in the previous chapters. We shall now make the plot of *The Prince* manifest.

The Conquest of Italy

Chapter 1 of *The Prince* begins with what Machiavelli claims is an exhaustive enumeration of different sorts of states, which can be categorized generally as republics or principalities. In chapter 2, after setting aside "reasonings on republics because I have reasoned on them at length another time" (presumably in the *Discourses*), Machiavelli commences with a discussion of hereditary principalities. Hereditary states do not occupy Machiavelli for long since, he claims, "if such a prince is of ordinary industry, he will always maintain himself in his state unless there is an extraordinary and excessive force which deprives him of it." His example of such a prince, the duke of Ferrara (actually two dukes), already alerts us to the existence of the papacy, for according to Machiavelli, this duke did not succumb to the attack of either Venice (a republic) or Pope Julius II (pp. 6–7). This prince or these princes actually did lose their state, however, and were fortunate to be restored by others. Although Machiavelli follows tradition by calling hereditary princes "natural" in this context, there is perhaps something more "natural" about new princes who acquire states, because, as

Machiavelli informs us, the desire to acquire is a "very natural and ordinary thing" in a world marked more by change than by stability (pp. 14–15; cf. *Florentine Histories* 5.1; see also Pocock 1975, 158; Strauss 1958, 57).

Machiavelli turns in chapter 3 to the epochal event that changed the landscape of Italian politics—the invasion of Italy by the French in 1494 and again in 1499. Although the chapter is entitled "Of Mixed Principalities," it is actually an account of Louis XII's failure, through his 1499 invasion, to create a mixed state by adding much of Italy to his own kingdom. Machiavelli offers Louis advice on how he could have succeeded—or rather, since Louis was dead, he counsels anyone who would conquer Italy. He offers the same advice in his *Discourses* (1.23), hardly an act of simple patriotism. Mansfield suggests an explanation: "Much as he loved Florence and Italy, he is not fundamentally a city or national patriot. He is a patriot on behalf of humanity, seeking to protect men against outside forces, consequently a patriot of the home of human beings, the earth" (1981, 303). While Mansfield's explanation is ultimately in accord with our own, it does not account for Machiavelli's more immediate intentions as revealed in this context. Butterfield embraces the opposite extreme, seeing Machiavelli's advice to Louis as nothing more than a compendium of maxims of statecraft (1962, 30–31, 60). Machiavelli's concern with the historical dilemma confronting Italy is in fact the unstated theme animating chapters 3 and 4 of *The Prince*.

Louis entered France with the aid of the Venetians and, most importantly, with the collusion of Pope Alexander. Machiavelli thus reminds us of the reason Italy was subject to such divisions and invasions—the papacy. He speaks of the division of Italy in the chapter on ecclesiastical principalities, although he does not there reveal the cause of the division: "Before Charles, king of France, came into Italy, this province was under the dominion of the pope, the Venetians, the king of Naples, the duke of Milan, and the Florentines" (pp. 45–46). As noted, chapter 3 is purportedly on "mixed" principalities but is actually about Louis's failure to conquer Italy. Italy is similar to France, where "Burgundy, Brittany, Gascony, and Normandy" have "been with France for so long a time" (p. 9).[10] Like France, Italy itself is a "mixed" or "disparate" province (see p. 11). Despite this similarity, Italy would appear to differ from France in one important respect, namely, the residence of the pope (at least after the end of the "captivity" of Avignon). If chapter 3 contains Machiavelli's advice on how to acquire a mixed province like France or Italy, he expands his plan of conquest with implicit reference to the papacy in the next chapter.

The subject of conquest continues in the apparently anomalous chapter 4, whose announced subject is, "Why the Kingdom of Darius Which Alexander Seized Did Not Rebel from His Successors after Alexander's Death." The conquest of Darius' kingdom was maintained because it was governed in a particular "mode," namely, by one prince whose ministers owe their power to him rather than by a prince surrounded by barons with hereditary privileges and their own subjects. Machiavelli states that "principalities of which memory remains" have been governed through one of these two modes. He presents a contemporary parallel to Darius' kingdom through the government of the Turk and contrasts it with that of the king of France. He explains: "Whoever considers the one and other of these states will find difficulty in acquiring the state of the Turk, but should it be conquered, great ease in holding it. So inversely, you will find in some respects more ease in seizing the state of France, but great difficulty in holding it" (pp. 17–18). Machiavelli advises one how to acquire both the state of France and that of the Turk. We have already seen that he considers Italy to be like France, so his remarks on the conquest of France would apply to Italy as well, with the notable exception of the papacy.

The papacy, in turn, is similar to the government of the Turk. Machiavelli has already furnished us with the information necessary to categorize the papacy when he remarked in the previous chapter that one reason Louis kept faith with the Pontiff was that Pope Alexander had undertaken to elevate one of Louis's ministers to cardinal (p. 15). The pope makes his own ministers, who therefore have no independent source of rule, just like the ministers of the Turk. The result of this comparison would be heartening to anyone contemplating an attack on the papacy: like the dominion of the Turk, this ecclesiastical principality is difficult to conquer but easy to hold after the initial assault. Machiavelli comments that once the Turk has been defeated, "one has only to fear the blood line of the prince" (p. 18). This statement, however, points to the manner in which the papacy must be distinguished from the Turk's state and Darius' kingdom of old: the papacy does not have a (recognized) blood line. The pope's authority derives from election. Later in *The Prince*, Machiavelli states that the sultan's state shares this feature with "the Christian pontificate" (p. 82). (Further, in this context, Machiavelli groups the Turk and the Sultan because of their dependence on their armies rather than on the people, a feature that the Christian pontificate shares in its dependence on mercenary arms.) The Christian pontificate is similar to the states of both the Turk and the Sultan.

Now in the case of Pope Alexander, Cesare Borgia himself represented the blood line, and so the pontificate under Alexander was more similar to the government of the Turk. Because his father ruled from the Vatican, however, Cesare was not the apparent heir to his father's rule. Indeed, this very circumstance would necessitate that Cesare, the natural son of the pope, conquer his father's principality in order to rule it. Burckhardt, for example, detects in Cesare's actions a plan to succeed his father as pope and to secularize the lands of the Church (1958, 129–30). Machiavelli draws our attention to the peculiar situation of the pope and his son at the very end of chapter 3, when he is relating a discussion with

Rouen that took place "when Valentino (for so Cesare Borgia, son of Pope Alexander, was called by the people) was occupying Romagna" (p. 16). Immediately after making this remark about Alexander and Cesare, Machiavelli turns in chapter 4 to Alexander the Great and his successors, causing us to think of the possible successor of a different Alexander from the one mentioned (twice) in the title of chapter 4.[11] Alexander the Great seized the kingdom of Darius, and his successors held it; Alexander Borgia attained the papacy, but *his* successor never gained possession of it.

Machiavelli provided a plan in chapters 3 and 4 of *The Prince* to conquer the whole of Italy, both the part resembling France and that resembling the sultan and the Turk. Cesare was more successful in conquering the part of Italy akin to France—eliminating the nobles and thus gaining the people to himself, "since they had begun to taste well-being" (p. 29),[12] an accomplishment to which Machiavelli appears to refer at the end of chapter 3 when he relates that Cesare was called Valentino by the people. (Actually, he was Duke Valentino, or Valentinois, as a vassal to the French king.) However, he did not succeed in acquiring his father's principality, a task that would have required overcoming those features of the papacy that make it similar to the Sultan and Turk's states. Cesare had the rare opportunity to accomplish the deed Machiavelli suggests, but he failed because he did not grasp the character of his father's realm or the deeds necessary to acquire it.

Finally, in chapter 5, Machiavelli offers advice on how to administer "cities or principalities" that lived under their own laws: "In truth there is no secure mode to possess them other than to ruin them"—or, he adds, to live in them (pp. 20–21). Such is the course Cesare would have had to take with Florence or any other republic in Italy, although we shall take up the relationship between princes and republics at greater length later. Machiavelli's plan for the conquest of Italy is complete by the end of chapter 5.

Virtue and Fortune, Crime and Opportunity

The play between virtue and fortune animates *The Prince* as a whole, and it is in chapters 6 and 7 of the work that the contrast comes to life. Speaking of "principalities that are altogether new in prince and in state," Machiavelli alerts us in chapter 6 that he will speak of "the greatest examples." He exhorts "a prudent man" to imitate such examples "so that if his own virtue does not reach that far, it is at least in the odor of it." Although the greatest examples have relied on their own virtue and arms, fortune or opportunity is also needed. Machiavelli writes of his great examples that "their excellent virtue enabled the opportunity to be recognized," but maintenance of the acquisition requires virtue above all. Among such princes, Machiavelli avers that "the most excellent are Moses, Cyrus, Romulus, Theseus, and the like" (pp. 21–23). As the founder of both a religion and a state, a combination Machiavelli esteems most highly (see *Discourses* 1.10), Moses is perhaps the most excellent of princes. Machiavelli initially claims to be reluctant to speak of Moses—just as he later states that "it would be the office of a presumptuous and foolhardy man to discourse" on ecclesiastical principalities (p. 45)—before proceeding to do just that. "And although one should not reason about Moses, as he was a mere executor of things that had been ordered for him by God, nonetheless he should be admired if only for that grace which made him deserving of speaking with God"—who, Machiavelli laconically remarks, was "so great a teacher" (pp. 22–23). What God taught Moses is unclear, but what Machiavelli seems to admire about Moses is his having relied on his own arms. He contrasts Moses to Savonarola, who also claimed to speak with God but was an "unarmed" prophet and therefore unsuccessful (p. 24). In order to understand Moses' accomplishment, as well as the power of the ecclesiastical dominion of Rome (a dominion founded by Jesus, a successful unarmed prophet, thus requiring us to amend Machiavelli's estimation of such prophets [see Berlin 1980, 64]), Machiavelli indicates that we must transcend our usual pieties and look at the "effectual truth of the thing."

Machiavelli provides, with chapter 7, "Of New Principalities That Are Acquired by Others' Arms and Fortune," the antistrophe to his discussion of princes who acquired through their own virtue and arms. Cesare Borgia serves as his primary example. "Those who become princes from private individual solely by fortune become so with little trouble," Machiavelli begins the chapter, "but maintain themselves with much" (p. 25). We see then that new principalities acquired by fortune are like France, which is easy to enter but difficult to hold, and, as we might expect, those acquired by virtue are like the Turk: those "who become princes by the paths of virtue, acquire their principality with difficulty but hold it with ease" (p. 23). We are reminded at the outset of the chapter concerned with Cesare Borgia of the discussion in chapter 4 of how Alexander's successors succeeded in holding the state he acquired.[13] Machiavelli briefly adduces Francesco Sforza as an example of a prince who acquired "with a great virtue of his own" but passes straightaway to Duke Valentino, so-called "by the vulgar" (thus reminding us of chapter 3). Machiavelli introduces Cesare by saying that he "acquired his state through the fortune of his father and lost it through the same, notwithstanding the fact that he made use of every deed and did all those things that should be done by a prudent and virtuous man to put his roots in the states that the arms and fortune of others had given him." Machiavelli claims, "I do not know what better teaching I could give to a new prince than the example of his actions" (pp. 26–27). Cesare is presented as an example but not one of the greatest kind. Again we are faced with the question of what "deed" Cesare omitted or what he could have done to raise himself to Machiavelli's highest rank.

Cesare's career flourished under the sponsorship of his father, who "decided to make his son the duke great." By facilitating the entry of the French into Italy, Alexander upset the existing "orders" and created the "disorder" necessary for his undertaking. Cesare thus acquired the Romagna, which served as the base for his subsequent acquisitions. Machiavelli praises in particular the Duke's decision "to depend no longer on the arms and fortune of others," thus dispensing with "auxiliary arms" (the French) and then mercenary ones (the Orsini) in favor of his own (pp. 27–28, see also p. 55). In this connection Machiavelli praises Cesare's turn to deceit to eliminate those on whom he had formerly depended and savors the Duke's elimination of these "heads" at Sinigaglia, the action that also "gained all those peoples to himself since they had begun to taste well-being."[14] He points to this action as "deserving of notice and of being imitated by others" and admires in particular the manner in which Cesare brought order to the Romagna through his minister, "Messer Remirro de Orco, a cruel and ready man, to whom he gave the fullest power." After Remirro succeeded in reducing the province to peace "with the very greatest reputation for himself," Machiavelli claims that Cesare judged that such excessive authority might become "hateful," and "in order to gain [the people] entirely to himself, he wished to show that if any cruelty had been committed, this had not come from him but from the harsh nature of his minister." Cesare therefore had Remirro killed in such a way that "the ferocity of this spectacle," Machiavelli comments, "left the people at once satisfied and stupefied" (pp. 28–30). Cesare was thus able to avoid being hated while retaining the people's love and fear, in accordance with Machiavelli's advice in chapter 19 of *The Prince*.

Machiavelli relates the Duke's deeds admiringly, but when he comes to Cesare's plans to maintain his acquisitions, the account becomes critical. Cesare's ultimate failure was that despite his resolve, he never dispensed with his reliance on the fortune of others. Machiavelli notes that Cesare had foremost to fear "that a new successor in the Church might not be friendly to him and might seek to take away what Alexander had given him" and speaks of the four "modes" in which Cesare sought to secure himself against this: eliminating the blood lines of those he had despoiled, winning over the gentlemen in Rome to keep the pope in check, making "the College of Cardinals as much his as he could," and acquiring sufficient "empire before the pope died that he could resist a first attack on his own." He almost succeeded in these plans, according to Machiavelli, and then "would no longer have depended on the fortune and force of someone else, but on his own power and virtue." His fatal mistake was that he used his influence in the College of Cardinals to make Julius II pope (pp. 30–32). Machiavelli's discussion reveals generally that Cesare erred by continuing to rely on his alliance with the Church. He relates in his legations that Cesare supported Julius, despite the injuries he had done him, because of Julius' assurance that he would appoint him as papal general, continuing the relationship with the papacy that he enjoyed under his father (1989, 1:155; see Guicciardini 1969, 174–6). Cesare kept faith with Julius, but according to Machiavelli, Cesare not only again relied upon the fortune of others but deceived himself (p. 33; see *Discourses* 3.4).

What should Cesare have done? Machiavelli indicates a more promising strategy in his relation of Cesare's failure. First, Machiavelli states of Cesare that "if he could not make pope whomever he wanted, at least it would not be someone he did not want" (p. 32). Initially, it appears that Machiavelli indicts him merely for his choice, but his restatement of Cesare's position a page later reveals another dimension: "One could only indict him in the creation of Julius as pontiff, in which he made a bad choice; for, as was said, . . . he could have kept anyone from being pope" (p. 33). Sasso notices that this is *not* what Machiavelli had said initially and concludes that the difference between the statements lies in the second's emphasis on the poor use Borgia actually made of his influence (1966, 147). Sasso is correct in characterizing the second as more negative; however, as we have argued, it also reveals what, in Machiavelli's view, Cesare should have done. Machiavelli says that Cesare could have kept *anyone* from being pope. This sentence can obviously be interpreted to mean that he could have kept any one candidate from being pope, but on another reading it signifies that Cesare could have ended the papacy altogether.

Machiavelli continues to reveal his plot in chapters 8 and 9, which parallel chapters 6 and 7, respectively. Chapter 8 concerns those who have attained a principality through "crimes," although Machiavelli equivocates on whether these "crimes" are actually acts of virtue. For instance, his first example in the chapter is Agathocles of Sicily, about whom Machiavelli says that "whoever might consider the actions and virtue of this man will see nothing or little that can be attributed to fortune," much like the greatest examples of chapter 6. Having spoken of Agathocles' virtue, Machiavelli retracts his attribution—"Yet one cannot *call* it virtue to kill one's citizens, betray one's friends, to be without faith, without mercy, without religion; these modes can enable one to acquire empire, but not glory"—only to grant it again: "For, if one considers the virtue of Agathocles . . ." (p. 35; emphasis added). Machiavelli later distinguishes what are "called" virtues and what virtue actually requires, with particular emphasis on keeping faith (pp. 61–62 and chap. 18, esp. 70–71).

Numerous scholars have taken Machiavelli's temporary refusal to call Agathocles virtuous to be his final opinion and have thus palliated or obscured the full force of the revolution he proposes in morality (see Pitkin 1984, 60–61; Skinner 1978, 1:137–38; see also Tarcov 1982, esp. 705–7). Machiavelli neither divorces politics from morality (as suggested by, e.g. Chabod 1964, esp. 243–55; Croce 1945, 250–56; Figgis 1960, 94–121) nor embraces half-heartedly or completely a political morality in tension with an unrejected Christian morality (as suggested by Berlin 1980), nor proposes an "economy of violence" (as suggested by Wolin 1960, chap. 7; see also Orwin 1978, 1225). His ultimate objection to Agathocles would appear to be to what he calls "his savage cruelty and inhumanity," his failure to employ necessarily violent means toward ultimately humane ends (p. 35). There are "cruelties badly used or well used," Machiavelli comments later on concerning Agathocles: "Those can be called well used," (if it is permissible to speak well of evil) that are done at a stroke [*a uno tratto*], out of the necessity to secure oneself" and are "turned to as much utility for the subjects as one can" (pp. 37–38 [Machiavelli 1971, 270]). He later praises Cesare for such cruelty as was actually "merciful" (p. 65). Machiavelli does indeed speak well of "evil." He announces at the beginning of his discussion of virtues, "I depart from the orders of others" (p. 61), and his departure entails a wholesale revision in the concept of virtue (see, esp., Orwin 1978; see also F. Gilbert 1977, chap. 2). When we appreciate the magnitude of his project, we are in a position to take seriously the claim that the "crimes" of Agathocles and others may potentially be acts of virtue.

Machiavelli's other primary example in his chapter on those who attain a principality through crimes is Liverotto (or Oliverotto) da Fermo, and it is Liverotto who serves as a partial pattern for what Machiavelli implies Cesare ought to have done. Machiavelli begins his account of Liverotto's history by calling attention to the parallel he will establish between Liverotto and his "father" on the one hand and Cesare and Alexander on the other: "In our times, during the reign of Alexander VI, Liverotto da Fermo, having been left a fatherless child some years before, was brought up by a maternal uncle of his" (p. 35). Machiavelli specifies that the events he relates here took place during Alexander's reign even though most of the modern events recounted in *The Prince* take place during that same time without his calling attention to the fact. After becoming an accomplished *condottiero,* Liverotto returned to Fermo and there "he held a most solemn banquet to which he invited Giovanni Fogliani," his uncle, "and all the first men of Fermo." Seemingly recalling his description of Cesare's conspiracy at Sinigaglia, Machiavelli relates, "Liverotto, with cunning, opened serious discussions, speaking of the greatness of Pope Alexander and of Cesare Borgia, his son, and of their undertakings." Again, our attention is drawn to the parallel. Adjourning to a more secret place, Liverotto had his uncle and the others killed and then seized power in the city. Machiavelli at first calls this act a "homicide," but later speaks of "the parricide he committed," and he does so when relating how Liverotto was strangled one year after his crime by Cesare at Sinigaglia (pp. 35–37; see also Guicciardini 1970, 234). Machiavelli forces us to compare Liverotto and Cesare and thereby suggests that Cesare should have seized his patrimony through patricide (see also Pitkin 1984, 60–63).

Papacide and the College of Cardinals

In Cesare's case, however, Liverotto's example alone could not suffice. Patricide would result only in the election of a new pope. The cardinals must also be eliminated at the time of his father's death. The cardinals comprise a sort of nobility surrounding the pope. Machiavelli takes up the question of how to deal with the nobility in chapter 9 of *The Prince,* "Of the Civil Principality." Just as chapter 8 on crimes is parallel to chapter 5 on virtue, so Machiavelli indicates at the outset of chapter 9 that it is parallel to chapter 7 on fortune: in a civil principality, "neither all virtue nor all fortune is necessary to attain it, but rather a fortunate astuteness." The chief lesson of this chapter is that in every city there are two "diverse humors," the "great" and the people: "A prince can never secure himself against a hostile people, as they are too many; against the great, he can secure himself, as they are few." Similarly, "one cannot satisfy the great with decency and without injury to others, but one can satisfy the people" (p. 39). Machiavelli thus concludes that "when a prince who founds on the people knows how to command and is a man full of heart . . . and with his spirit and his orders keeps the generality of people inspired, he will never find himself deceived by them and he will see he has laid his foundations well" (p. 41). Machiavelli has already noted several times that Cesare was founding himself on the people and opines that "his foundations were good" (p. 32). One way to deal with nobles is to destroy them. For example, in the *Discourses* Machiavelli relates how Clearchus, "finding himself between the arrogance of the aristocrats, whom he could in no way satisfy or control, and the rage of the citizens," took a suitable opportunity to solve his problem "at one blow"(*a un tratto*): "He cut to pieces all the aristocrats—to the utter satisfaction of the people" (1.16 [Machiavelli 1971, 100]; see also Langton 1987, 1280). Cesare was accomplished at eliminating the great, as Machiavelli's macabre admiration of his exploits at Sinigaglia shows. What Cesare needed to do was to eliminate another group of "heads," the cardinals. Machiavelli does not, of course, specify that the destruction of the clerical aristocracy would, in this way, result in a more stable rule for a prince. Nevertheless, it appears justified to speculate that the destruction of a class of people "who without working [*oziosi*] live in luxury on the returns from their landed possessions" would satisfy the people (*Discourses* 1.55 [Machiavelli 1971, 138]).[15] The elimination of the cardinals was the only way that Cesare could maintain his rule.

As the son of the pope on whom his father's ambitions were focused, Cesare had enviable access to the College of Cardinals. Indeed, Guicciardini reports that Alexander's death resulted from an accidental poisoning at a dinner that he and Cesare hosted for a number of cardinals. Alexander "had arranged a large garden party, at which he planned to poison a few cardinals so that he could then sell their offices and their benefices." Cesare and Alexander arrived early and asked for some wine to relieve their thirst. They were served the poisoned wine inadvertently: "That this is true is attested by the fact that he died either the same night or the next day; and by the fact the Valentino and a few others who were there fell into a long and critical illness, with signs of poisoning" (Guicciardini 1970, 240–41; see also idem 1969, 165–6).[16]

Machiavelli attributes Cesare's downfall to "an extraordinary and extreme malignity of fortune." He relates that Cesare himself told him on the day that Julius II was created "that he had thought about what might happen when his father was dying, and had found a remedy for everything, except that he never thought that at this death he himself would also be on the point of dying" (pp. 27, 32). As it stands, Machiavelli's report of Cesare's comment is simply misleading (see Mattingly 1958, 488). He in fact lived another three years and died in battle! Furthermore, after the college's first choice as pope, Pius III, died only a month after his election, Cesare was vigorous enough to exert his influence with the Spanish cardinals to assure Julius's ascendancy (Machiavelli, 1989, 1:143, 149; see also Guicciardini 1970, 245).[17] According to Machiavelli's analysis, for Cesare to succeed he had to exert independence not only from his father but from the Church. Machiavelli appears to acknowledge the impossibility of Cesare's coexistence with a new pope: "He could not make a pope to suit himself" (p. 33). The lamentation that Machiavelli ascribes to Cesare appears to be his own analysis, rather than that of the historical Cesare, for in his legations, written contemporaneously with these events, Machiavelli characterizes the Duke as incapable, at the time of Julius' ascension, of the astuteness required for this insight (1989, 1:146). Only because Cesare had tied his fortune to that of his father can he be said to be "on the point of dying" at that time. Cesare deceived himself not only about Julius' faith but also about himself. Ultimately, like Baglioni, Cesare is to be censured because he lacked the insight to capitalize on his criminal character and thus to become truly great.

The Prince constitutes an assault on the power of the Church and of Christianity generally. The Church of Machiavelli's time did appear "secure and prosperous;" and indeed, he notes that it is upheld "by superior causes" and claims that "it would be the office of a presumptuous and foolhardy man to discourse on them" before doing just that (p. 45). Machiavelli's departure from the orders of others requires overcoming "the incredulity of men, who do not truly believe in new things unless they come to have a firm experience of them" (pp. 23–24). In tempting his readers to unravel the plot featuring Cesare Borgia, Machiavelli induces them to acquire the knowledge that the ecclesiastical power is vulnerable as well as inimical. The plot of *The Prince*, then, is itself the inspiration to wage the necessary assault on that power.

WHAT MACHIAVELLI RENDERS UNTO CESARE

Machiavelli's criticism of Cesare Borgia's reliance on the fortune of others is related to his treatment of fortune and virtue more generally. If Guicciardini can celebrate the downfall of the Borgias by acknowledging the ultimate "justice and power of God, whose boundless might cannot be contained within the narrow limits of the present" (1969, 166), Machiavelli's interest lies elsewhere. He opposes the opinion that "worldly things are so governed by fortune and by God, that men cannot correct them with their prudence" (p. 98). Machiavelli's ultimate intention is to manage—if not to conquer—fortune through the power of virtue. Christianity has made the world weak, Machiavelli claims (*Discourses* 2.2). His project requires reasserting human autonomy by cutting a chain of instrumentality that is seen by Christian eyes to stretch from God through earthly powers to all people.

The case of Cesare Borgia presents both the power of the chain of dependence Machiavelli would break and the possibility of doing so. Cesare himself gave his minister, Remirro, "the fullest power" to pacify the Romagna.[18] When Remirro became dangerous Cesare had him eliminated, leaving him "one morning in the piazza at Cesena in two pieces, with a piece of wood and a bloody knife beside him" (pp. 29–30; see Machiavelli 1989, 1:141–42). In recounting this harrowing tale, Machiavelli imparts a lesson regarding instrumentality. Cesare did well in this instance by manipulating his minister. Machiavelli makes it clear that Remirro's deeds in fact belonged to Cesare. However, later we find that according to Machiavelli's analysis, Cesare is not the master manipulator but is himself manipulated. Just as Remirro's deeds belonged to Cesare, ultimately Cesare's deeds belonged to his father. Later Machiavelli clarifies Cesare's position when delineating Alexander's deeds: "With Duke Valentino as his instrument and with the invasion of the French as the opportunity, he did all the things I discussed above in the actions of the duke" (p. 46). Alexander, not Cesare, is the actor.

Machiavelli's analysis does not end there, however, for it is revealed that Alexander too was an instrument: "Though his intent might not have been to make the Church great, but rather the duke, nonetheless what he did redounded to the greatness of the Church. After his death, the duke being eliminated, the Church fell heir to his labors" (pp. 46–47). As Pocock recognizes, Cesare's power "remains wholly dependent on papal and curial politics and Machiavelli was unable to assert convincingly that it does not" (1975, 174). However, according to our interpretation, Machiavelli did not want to establish Cesare's independence from the Church, because Cesare's failure in this regard provides the very basis of Machiavelli's dissatisfaction with Cesare's actions.

Machiavelli's chain of instrumentality does not end with the terrestrial Church. Machiavelli claims that ecclesiastical principalities "subsist by superior causes" and "are exalted and maintained by God" (p. 45). Ultimately, then, God stands at the head of this chain of instrumentality. Cesare finds himself near the bottom of a rather considerable chain of instruments, and as a result he ends in a position no better than that of his own minister. As Machiavelli writes in his first *Decennale*, "When Alexander was slain by Heaven [*ciel*], the state of his Duke of Valence was broken and divided into many pieces" (1989, 3:1455 [Machiavelli 1971, 948]). The debate over the relative power of the temporal and spiritual powers that so occupied his immediate predecessors was certainly pernicious, as Machiavelli makes clear in his account of the tumult in Florence due to the struggle between the Guelfs and Ghibellines (see *Florentine Histories*, esp. bks. 2–3). For Machiavelli, however, this debate is ultimately strictly secondary in importance. The positions of both the spiritual and the secular powers, as well as those who would mediate them, all ultimately founded themselves on a theory that power is derived from God (see Gierke 1987, esp. 7–20, 30–32). As long as one is forced to render unto Caesar what is Caesar's and to God what is God's, human beings cannot enjoy a true civil life. Machiavelli ushers in modern politics by suggesting that a new prince break the chain of instrumentality through his own arms and virtue. Cesare Borgia had the opportunity to do just that, and Machiavelli offers him as an example to one who would not omit the necessary acts. Fortune must be broken on virtue's wheel.

MACHIAVELLI'S POLITICAL VISION

After Machiavelli divulges the last crucial elements of his plot in chapter 11 of *The Prince*, the theme of Italy emerges in the succeeding chapter when he informs us that the Church has disarmed the country and made it weak. Italy's fate depends on the plunderous whims of others. *The Prince* concludes with the stirring exhortation for the redemption of Italy from its humiliation at the hands of foreigners: "Left as if lifeless, she awaits whoever it can be that will heal her . . . of her sores that have festered now for a long time. One may see how she prays God to send her someone to redeem her from these barbarous cruelties and insults" (p. 102). In the *Discourses*, Machiavelli argues that it is the Church that "keeps this region divided" and adds that "truly no region is ever united or happy if all of it is not under the sway of one republic or one prince, as happened to France and to Spain" (1.12). We have seen that the same line of reasoning informs his considerations in *The Prince*. The redemption of Italy requires its unification and therefore the destruction of the Church.

So great do some commentators find the disjunction between the impassioned patriotic plea of chapter 26 and the analytic ruthlessness of the treatise's previous chapters that they conclude that the last chapter is not in fact a part of the original work (e.g., Baron 1991). Others note this disparity but claim that this apparent gulf must be bridged: his overt immorality must be read in light of his patriotism. Now, having unraveled its plot, we see precisely what Machiavelli's patriotism must serve to forgive (see Strauss 1958, 81): the rectification of Cesare's errors would require the commission of unspeakable crimes.

If the theme of the ruinous effects of the Church on temporal affairs unites *The Prince* and the *Discourses*, what are we to make of the more general and intractable problem of Machiavelli's declared preference for republics on the one hand and his willingness to advise princes on the other? Viroli, for example, presses the division between the two works to the point where he denies that *The Prince* is even about politics: "Princely rule, be it hereditary or newly founded, cannot in any sense be equated with the *civitas*, and the art of preserving princely rule does not coincide with the art of instituting or preserving a *vivere politico*;" in *The Prince*, he claims, Machiavelli "was not writing about politics as he understood the term" (1990, 161). This position is simply too extreme. For example, in his work supposedly on republics, Machiavelli confirms that Romulus—a single man and a king—established a *vivere civile* ("civil life," *Discourses* 1.9). Machiavelli does not limit the term *vivere civile* to republics, for Romulus introduced kingship, not republican government, to Rome. A *vivere politico* can occur "by way of republic or by kingdom" (1.25 [Machiavelli 1971, 109]; see 1.55). Machiavelli thus does not preclude the possibility of a political life emerging from a state with a single head. Indeed, many scholars have paid heed to Machiavelli's claim that it is necessary to be alone to found a republic (e.g., Wolin 1960, 231–32).

Dietz is skeptical about whether this princely rule, or "heroic politics," will "somehow 'give way' to mass politics" (1986, 780–81). She neglects considerations that would, while affirming her understanding of Machiavelli's pessimistic view of human nature, nonetheless lead to another conclusion. Even from the *Discourses* it is unclear that Machiavelli desires the emergence of "mass politics," as such. Although he favors the Roman republic, which expanded effectively because it allowed the plebeians a voice in the regime, Machiavelli is quite careful not to expunge princely or heroic rule from his depiction of this exemplary republic. For example, he includes among the benefits of a free way of life the knowledge that one's children "by means of their abilities . . . can become princes [*principi*]" (*Discourses* 2.2 [Machiavelli 1971, 150]). Not only does he insist upon calling the leading men of the Roman republic "princes," but he shows how the devious maneuverings of these leading men kept the mass of citizens from exercising control in the regime (1.47–48, 3.11). Moreover, he shows how a regime can overcome the problem endemic and dangerous to republics, that of slowness to act (1.59, 3.6), by infusing itself with the resoluteness, even the despotic character, of princely regimes (3.1). Machiavelli does not consider princes and republics to be completely separate or even contradictory in nature.

The deed of founding a republic need not be as altruistic an act as Dietz suggests. Machiavelli holds out the promise of fame to the one willing to undertake the travails of such a founding (1.9). The vigor of such a republic serves to exalt the memory of its original legislator. Machiavelli suggests that founding—and refounding—harnesses the tyrannical impulses of leading men. Further, the reflection that a founder of a free way of life must arrogate all authority to himself is far from unusual in political thought. Rousseau, whose credentials as a republican or democratic theorist are, to say the least, less assailable than those of Machiavelli, follows his Florentine predecessor in regarding the lone legislator as essential for the foundation of a free state. Like Machiavelli, the Citizen of Geneva holds out fame as the reward for legislators.

The notion informing much scholarship that Machiavelli's treatments of republics and of princes are analytically wholly distinct is simply incorrect and has placed unnecessary obstacles in the way of ascertaining Machiavelli's political vision. Machiavelli calls on a prince with both the virtue and fortune to remedy Italy's ills and thus to provide the conditions necessary for a reinvigorated political life in Italy and modern times more generally. Our interpretation of Machiavelli's more immediate intentions in *The Prince* is therefore also a first step needed for a consideration of his ultimate intentions.

If one needs one's own arms and virtue to establish a new principality, Machiavelli laments his fortune and his lack of arms. Yet armed with his pen he may address a virtuous prince who also possesses the opportunity and arms, whether an actual prince or those who deserve to be princes. Machiavelli is an "unarmed prophet" who unabashedly announces in *The Prince*, as well as in the *Discourses*, that he departs from the modes and orders of others (1985, 61; *Discourses* 1, pref.; see 1.55). *The Prince* contains his demand that someone with the virtue and fortune pick up the sword and do what Machiavelli is unable to complete (see *Discourses*, 2, pref.). He counsels another through his words to perform the deeds he cannot. Machiavelli will become a great teacher for the one who acts as his instrument, but in a fashion that will not derogate from the fame of that truly virtuous prince. Moses is Machiavelli's most outstanding example of a prince who founds a state and orders a religion through his own virtue. Moses' accomplishment required that he seize the opportunity to display his virtue, and Machiavelli explains, "He who reads the Bible intelligently [*sensatamente*] sees that if Moses was to put his laws and regulations [*ordini*] into effect, he was forced to kill countless men who, moved by nothing else than envy, were opposed to his plans" (*Discourses* 3.30 [Machiavelli 1971, 237]). Whoever reads Machiavelli's *Prince* in a similar manner and understands it will see that he too must arrange the deaths of a very great number of men. If their deaths will mean a reformation of political life, then we concur with Rousseau that *The Prince* is a work intended for "republicans."

Notes

We would like to thank Eduardo Velasquez, Roger D. Masters, Paul A. Rahe, and Joseph Alulis for their comments and suggestions.

1. We shall cite *The Prince* (1985) by page, and the *Discourses on the First Decade of Titus Livius* (1989) and the *Florentine Histories* (1988) by book and chapter. Where we cite the Italian, references are to Machiavelli 1971. All references in the text are to *The Prince* unless otherwise noted.

2. For a review of the literature on Machiavelli, see Berlin 1980, 25–39; Cochrane 1961; Geerken 1976. On Machiavelli's modernity, see, e.g., Figgis 1960, chap. 3; Mansfield 1981; Wolin 1960, chap. 7. For dissents, see A. Gilbert 1938; Parel 1992; Skinner 1978, 1:113–90; idem 1990).

3. Rousseau claims that Machiavelli's "hidden intention" is revealed by the contrast between the "maxims" of *The Prince* and those of his republican writings and says that "this profound political theorist has had only superficial or corrupt readers until now." Rousseau claims that Machiavelli was forced "to disguise his love of freedom" (*Social Contract* 3.6, n.). McKenzie examines Rousseau's rehabilitation of Machiavelli and concludes that he "found a way of coming to terms with *The Prince* but not with Machiavelli's republican theory . . . without first extracting those teeth which threatened to rend the fabric of his own republican theory" (1982, 226–28). McKenzie's analysis is helpful, but he at least underestimates the similarity of Rousseau's legislator to Machiavelli's prince. Rousseau cites Machiavelli in his chapter on the legislator in the *Social Contract* (2.7), and his examples of legislators here and elsewhere are the same individuals Machiavelli cites as princes by their own virtue in chapter 6 of *The Prince*. The kinship of Rousseau's legislator to Machiavelli's prince also brings our interpretation of Machiavelli closer to Rousseau's.

4. Felix Gilbert follows Meinecke in arguing that *The Prince* was composed in stages, the first stage being the first 11 chapters, the "little treatise" Machiavelli announces in the famous letter to Francesco Vettori (F. Gilbert 1977, 112–14; see Machiavelli 1989, 2:929). Whatever the merit of Gilbert's thesis, it at least suggests the coherence of the first half of the work and the need to read it with the discussion of the papacy in chapter 11 in mind. In contrast, Skinner (1981) does not discuss chapter 11 in his interpretation of *The Prince*.

5. Machiavelli appears to refer to Savonarola as the one who blamed Italy's invasion on its "sins," but he may also have Petrarch in mind. *The Prince* ends with the conclusion to Petrarch's patriotic poem "Italia Mia" but does not include Petrarch's lament over the divisions of Italy (divisions Machiavelli attributes primarily to the Church): "It is on account of our own sins, and not a natural thing that the slow northerners should conquer us in intellect" (Petrarch 1976, 260–61).

6. This application of Machiavelli's vituperation to the clergy is not intended to derogate from his obvious disgust for gentlemen conventionally understood (see *Discourses* 3.29; *Florentine Histories* 1.39).

7. Machiavelli is astonished that Baglioni, who possesses the heart of a criminal, could not commit this deed. As evidence of this criminality Machiavelli adduces the claims that Baglioni committed incest with his sister and killed his cousins and nephews in order to rule (*Discourses* 1.27). Machiavelli's characterization of Baglioni shares a striking kinship with the historical Cesare Borgia. Guicciardini reports that it was believed that Cesare had had an incestuous affair with his sister, Lucrezia (1970, 197). Burckhardt relates that in order to have his father's territorial ambitions redound solely to him, Cesare murdered his brother, brother-in-law, and other relations (1958, 129).

8. Machiavelli's account of Romulus' founding of Rome departs from that of Augustine, who views his fratricide as an indictment of politics in a fallen world (*De civitate Dei* 15.5, see also 3.6; cf. Livy, *De urbe condita* 1.6). In addition, Machiavelli appears to exaggerate Romulus' crimes in comparison to Livy. Whereas Livy says merely that Romulus declined to go to war with those responsible for his colleague's death (1.14), Machi-

avelli declares that Romulus was "a party to the death of Titus Tatius" (*Discourses* 1.9). Perhaps this emphasis derives from Machiavelli's recognition of the magnitude of the crime that is necessary to found a new Rome.

9. Although Machiavelli speaks in *The Prince* of the "dispersal" of the Athenians as the occasion for Theseus to show his virtue (p. 23), it was in fact upon his return from killing the Minotaur that Theseus actually seized the occasion: he failed to raise a white flag (as promised to herald his safe return), and his father, King Aegeus, leapt to his death. Plutarch relates the story in his *Life of Theseus*, Machiavelli's source, and says of Theseus that "after the death of his father Aegeus, forming in his mind a great and wonderful design, he gathered together all the inhabitants of Attica into one town, and made them one people of one city, whereas before they lived dispersed" (n.d., 13–15, 48). Pitkin claims that "the Founder saves and protects, rather than slays, his father," discussing in this context the actual or potential parricides we mention as well: Baglioni, Liverotto da Fermo, and Cesare (1984, 60–63; see also Strauss 1958, 258).

10. Actually, several of these provinces had been added to the kingdom only quite recently. Modern scholars have tended to overstate Machiavelli's view of France when they claim that he found there a model for Italy, for France was not yet a fully unified realm, much less a nation-state (see Rubinstein 1990, 24–28; Skinner 1981, 6–7). The process of France's consolidation, which necessitated control over previously independent "barons," is described by Machiavelli in his *Ritratti delle cose della francia* (1971, 56). Brittany, for example, was acquired only in 1491, when Louis XII obtained an annulment from his first wife to marry Anne of Brittany, who carried the dukedom as her dowry. The annulment was granted by Pope Alexander and delivered by Cesare, who in payment obtained the duchy of the Valentinois and the hand of Charlotte d'Albret, the king's niece (Machiavelli 1985, 15, 27–28; see Guicciardini 1970, 150). A similar arrangement was not forthcoming for Henry VIII of England not long thereafter.

11. Machiavelli seems to play on the fact that the names of the pope and his son replicate those of these great historical figures; hence, Cesare is both "Cesare" and "Caesar" in the scholarly literature. For example, in *The Prince*, in a context that clearly applies to Julius Caesar, Machiavelli claims that *e Cesare era uno di quelli che voleva pervenire al principato di Roma* (p. 64 [Machiavelli 1971, 281]). The original Italian, removed from its immediate context, might apply to either Borgia or Julius. Further, if Caesar aspired to a "principate," Cesare attempted to acquire an "empire" (*imperio*) before the pope died (p. 31 [Machiavelli 1971, 268]). Gentillet notes the possible ambiguity of this last citation and says of Cesare: "Either Ceasar [*Cesar*] or nothing: as though to say that he esteemed nothing less than being lord as was Julius Caesar [*Cesar*]" (1968, 340). See *Florentine Histories*: "Men too, once Caesars [*Cesari*] and Pompeys, have become Peters, Johns, and Matthews" (1.5 [Machiavelli 1971, 637]).

12. In the *Discourses*, Machiavelli notes that "Pope Alexander VI destroyed the lords who ruled" the Romagna, thus eliminating "the most wicked ways of living" (3.29). Here the author ascribes Cesare's actions to his father, a conclusion he also draws in *The Prince* in a passage we shall discuss.

13. Should we have missed his initial reference, Machiavelli appears to reenforce it immediately when he likens new princes who acquire by fortune to those who "were made princes by Darius" (p. 26)—though this Darius is not the Darius of chapter 4, as noted by Mansfield in his edition (Machiavelli 1985, 26).

14. For the details of Cesare's deception, see Machiavelli's "Description of the Method Used by Duke Valentino in Killing Vitellozzo Vitelli, Oliverotto da Fermo, and Others" (1989, 1:163–69).

15. At one point in the *Discourses*, Machiavelli says that the number of great in any state never exceeds 40 or 50. Because the number is so low, a prince can kill them or honor them according to their standing and thereby satisfy them for the most part (1.16). Why does Machiavelli place such a definite number on a class that surely varies according to the size of the state? At least one body of nobles did satisfy this amount in Machiavelli's time—the College of Cardinals. Although the number of cardinals was set at 24 by the Councils of Constance (1418) and Basle (1436) (*Catholic Encyclopedia* s.v. "cardinal"), at least Popes Alexander and Julius disregarded the limit and raised money by selling addition cardinals' hats or sometimes by killing existing cardinals and selling their positions (Machiavelli 1985, 47; see Guicciardini 1970, 241). In 1503, 38 cardinals—the vast majority of them—convened to elect Pius III and then Julius II a month later (idem 1969, 170; idem 1970, 243).

16. The editor disputes this account and states that Alexander seems to have died of a malarial fever (Guicciardini 1970, 240). Frederick of Prussia accepts the account derived from Guicciardini, commenting of Alexander's death: "This is the prudence, wisdom, ability, and virtue that Machiavelli never tires of praising" (1981, 62). Burckhardt also accepts the account (1958, 128, 132). Whatever the validity of Guicciardini's account, Alexander and Cesare were widely known to have poisoned a great number of people, including cardinals. More importantly, Machiavelli appears to have been Guicciardini's source about Alexander and Cesare, since he repeats in his *History of Italy* (1969, 166) what Machiavelli relates in *The Prince* regarding Cesare's statement that he himself was "dying" at the same time as his father (p. 32), so that Machiavelli very likely accepted that Alexander had been accidentally poisoned.

17. Burckhardt seems to have been misled by Machiavelli's remark: "And what might not Cesare have achieved if, at the moment when his father died, he had not himself been laid upon a sick-bed! What a conclave would that have been, in which, armed with all his weapons, he had extorted his election from a college whose numbers he had judiciously reduced by poison—and this at a time when there was no French army at hand! In pursuing such an hypothesis the imagination loses itself in an abyss" (1958, 133).

18. Mansfield notes in his edition of Machiavelli's *Prince* (1985, 29) that Machiavelli's use of the term *pienissima potestà* in this context recalls the papal claim of *plenitudo potestatis*.

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RESEARCH NOTES

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COMPARATIVE DEMOCRACY: THE ECONOMIC DEVELOPMENT THESIS

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In comparative politics, an established finding—that economic development fosters democratic performance—has recently come under challenge. We counter this challenge with a dynamic pooled time series analysis of a major, but neglected data set from 131 nations. The final generalized least squares–autoregressive moving averages estimates (N = 2,096) appear robust and indicate strong economic development effects, dependent in part on the nation's position in the world system. For the first time, rather hard evidence is offered on the causal relationship between economics and democracy. According to Granger tests, economic development "causes" democracy, but democracy does not "cause" economic development. Overall, the various tests would seem to advance sharply the modeling of democratic performance.

Political sociology has few, if any, "iron laws." However, certain central hypotheses seem so established as to be almost beyond challenge. According to Lipset "Perhaps the most widespread generalization linking political systems to other aspects of society has been that democracy is related to the state of economic development" (1959, 75). Among the many reasons offered for this empirical connection, a common idea is that increasing economic benefits for the masses intensify demands for the political benefits of democracy. Economic development can spread authority and democratic aspirations among a variety of people, thus fostering democracy (Dahl 1989). The notion of economic development as a "requisite" to democracy (Lipset 1959) has survived increasingly sophisticated statistical tests.

Leading relevant quantitative, multivariate studies in the political science and sociology literature include Jackman (1973), Bollen (1979, 1983), Bollen and Jackman (1985) and Brunk, Caldeira, and Lewis-Beck (1987). In each of these regression-based analyses, data are gathered on a sample of nations, and a nondichotomous measure of democratic performance is predicted from economic development and, usually, other independent variables. Economic development consistently emerges as a statistically and substantively significant influence on democracy. For example, Brunk, Caldeira, and Lewis-Beck recently found that economic development alone accounts for more variance in democracy than the other independent variables taken together (1987, 468).

Given the strong results of these varied regression analyses, one might imagine that the "economic development thesis" merited little further testing. However, these studies, careful as they are, have noteworthy limitations. The samples are rather small (average = 78 nations) and sometimes explicitly nonrepresentative. (Bollen and Jackman [1985], for example, exclude communist nations.) Further, they are all based on democracy measures that are by now quite old, from either 1960 or 1965. Finally, and most importantly, they are cross-sectional, not time-series, in design (though Bollen's [1979] study contains a measure from an earlier cross section). Of course, the absence of a temporal dimension makes it difficult to overcome the criticism that the positive economics–democracy connection represents a spurious association, occurring within a particular time-slice.

These data difficulties receive some remedy in Arat (1988) and Gonick and Rosh (1988), two more current quantitative democracy studies. Both use larger samples of nations (95 and 116, respectively), including communist ones. Moreover, they extend the measurement of democracy into the 1970s to 1977 and 1979, respectively). Of special note, in their model estimations, they explore a series of cross sections over time. Given the previous quantitative work, one might expect economic development to continue to exhibit strong, unrivaled, effects. But it does not. To quote Arat: "Only a few countries fit the models suggested by modernization theory. . . . It can be concluded that increasing levels of economic development do not necessarily lead to higher levels of democracy, even for the less developed countries" (1988, 30). The conclusions of Gonick and Rosh have a similar ring: "Economic development . . . is not the most important factor affecting the degree to which a political system can be characterized as a 'liberal democracy'. . . . Our application has allowed us to reject the findings of Lipset" (1988, 189, 196).

These surprisingly weak findings, based on apparently better data, force a reevaluation. Perhaps economic development actually has little influence on political democracy. Or, possibly, these analyses possess shortcomings that still leave the question open. Let us consider three potential problem areas: design, specification, and measurement. Although both studies examine cross sections over time, neither takes full advantage of the statistical leverage a pooled time-series design allows. Arat (1988) simply applies ordinary least squares (OLS) to the pool before discarding it for a country-by-country time-series examination. Gonick and Rosh apply a "variance component" model to the pool and neglect to discuss the central problem of autocorrelation from the time series (1988, 195). Nor does either explore lagged effects or Granger tests for causality. In other words, they ignore the dynamic, analytic opportunities that the time component of the pool affords.

With regard to specification, Arat does not include any independent variables other than economic development, a modeling strategy not followed after Jackman's (1973) initial baseline work (1988, 27). While Gonick and Rosh have an equation that is more fully specified, they include independent variables (e.g., urbanization, literacy) that the other quantitative studies do not include in their "direct effects" equations (1988, 194–95). This specification difference could generate an unusual amount of collinearity, perhaps accounting for their weaker findings on economic development effects.

With respect to measurement, they both rely heavily on the cross-national time-series data of Banks (1979). Arat (1988) uses the Banks data for five of the six scales in her democracy measure. Gonick and Rosh (1988), for their democracy measure, use Banks data exclusively. Of course, the Banks data have been widely used in quantitative democracy studies. The difficulty is that key Banks democracy indicators, as they are commonly used, contain considerable measurement error, as Bollen (1993) has recently shown.

Bollen (1993) employs structural equation modeling with latent variables to arrive at the estimated "validity component" of democracy variables that have been constructed from different data sources. (The "validity component" is what remains of the measure after the random measurement error and systematic measurement error components have been extracted. A variable with no measurement error could be assigned a validity component rating of 100%.) According to Bollen (1993), the validity component ratings on the Banks democracy variables he studied are as follows: chief executive elected (14%), competitiveness of the nomination process (62%), effectiveness of legislative body (79%), freedom of group opposition (92%). These four variables, with the possible exception of the last, contain serious measurement error. Gonick and Rosh (1988, 184) use all four variables in building their democracy index, while Arat (1988) uses the first three. Given the high degree of measurement error that is thus contained in their democracy indices, it becomes difficult to have confidence in their conclusions.

We shall try to overcome the foregoing research problems. We examine data on more nations, over a longer and more recent span of time. The data set is not Banks but, rather, Gastil. The research design takes full advantage of the pooled time series structure, ending with a generalized least squares–autoregressive moving averages (GLS–ARMA) estimation, with appropriate diagnostics along the way. Model specification builds on previous work, but introduces promising theoretical innovations that at the same time guarantee a rigorous test of the economic development thesis. Additionally, the issue of measurement error (which affects Gastil data as well) is dealt with. Finally, firm conclusions are drawn about the economic development thesis.

A DEMOCRACY INDEX: THE GASTIL DATA

Democracy can have many meanings. See the current provocative philosophical exchange between Mueller (1992) and Lienesch [1992]. Here we follow the tradition of the above quantitative studies, conceiving of democracy as a continuous variable, scored numerically from low values to high. The particular democracy measure we select aims to parallel the Bollen (1979) measure. Working for Freedom House, Gastil has provided annual (1972–89) ratings of nations on two dimensions (see Appendix A). The first, here labeled "political rights," assesses the right to vote, election meaningfulness, multiple political parties, opposition power, and government independence from foreign or military control. The second, here labeled "civil liberties," covers the freedoms of speech, assembly, and religion and freedom from terrorism or blatant inequality. (Details on the items appear in Appendix A.) By adding together the ratings on these two 7-point scales (after reversing their coded order for readability), each nation acquires an overall democracy index, ordered from a low of 2 to a high of 14.

The Gastil data have received considerable use in social science research (see Bollen 1993) but not for constructing an explanatory model of democracy either in cross-sectional or time-series studies. In certain respects, these data represent a splendid opportunity. Using them, we assembled annual observations on 131 nations 1972–89, yielding a total sample size of 2,358. (The nations and their democracy scores are listed in Appendix B.) Hence, it becomes possible to incorporate simultaneously analysis of over-time, as well as cross-nation, variation in democracy. Sources of variation that would have been absent in a single cross section are now captured. In sum, a pooled methodology offers the prospect of serious gains in statistical efficiency.

MODEL SPECIFICATION

In the seven studies at hand, there is little agreement on the proper independent variables, beyond the inclusion of economic development (E) itself, which is invariably postulated to have a curvilinear effect on democracy (D). A few variables do appear in more than one model: public control of the economy, Protestant population, historical timing of development, and *world-system position* (W). Each of these attains statistical significance in at least one model, which suggests that other social forces (S), as well, influence democratic performance. However, the only variable to achieve statistical significance at .05 in more than one study is world-system position (Bollen 1983; Bollen and Jackman 1985; Gonick and Rosh 1988). This suggests the conceptual specification:

$$D = f(E, S, W)$$

where D = democracy, E = economic development, S = other social forces; W = position in the world-system.

We now proceed to the operationalization of these independent variables. For *E*, we employ energy consumption per capita (logged), the economic development measure most often used, and one correlated .9 with gross national product per capita (Bollen 1979, 578). For *S*, we utilize a proxy, the lagged value of democracy, $Dt - 1$. The rationale is straightforward. The other social forces acting on democracy are uncertain (and perhaps unmeasurable across the pool). However, to the extent they are structural processes (e.g., public control of the economy, Protestant population, historical events), they will be essentially summarized in the democratic performance of the nation during its previous year. In other words, $Dt - 1$ acts to control for omitted independent variables. With such a pervasive control in place, it is more difficult for spurious economic effects to be reported. (Still, Bollen (1979) found economic development significantly related to democracy in a 1965 cross section, even after including the 1960 democracy index as a predictor.)

With regard to a measure for *W*, a few preliminaries are in order. The notion of a *world system* represents a major theoretical step forward in the literature. Accordingly, a nation's chances for democracy may depend on where it is placed within the world economy. (For the theoretical inspiration, see Wallerstein 1974.) The core economies (high wages, high profits, capital-intensive), are bound in a lopsided exchange with the periphery economies (low wages, low profits, and labor-intensive). The constraints imposed by this international economic system (so the argument goes) seriously diminish the likelihood of successful democracy in the periphery.

Operationalization of the world-system position differs somewhat from study to study, but always involves dummy variables for different sets of countries (e.g., semiperiphery *M* and periphery *P*) entered additively into the estimation equation. Sometimes, expected effects have fallen short of statistical significance. Further, even when significance is attained, the form of the effect is seriously misspecified.

The last point deserves amplification. Gonick and Rosh who are leading world-system effects advocates, argue that "the effects of economic development are fundamentally shaped by the country's position vis-à-vis the World-Economy" (1988, 195). This is an unambiguous argument for the world-system position's operating nonadditively. In particular, world-system position conditions the impact of economics on democracy. To incorporate this dependency, then, the preferred specification is an interaction of the world-system position dummies with the economic development variable. This specification should actually increase the likelihood of uncovering world-system effects. (Details on construction of the world-system categories appear in Appendix B.)

These considerations lead to the model

$$Dt = a + bDt - 1 + cEt + d(M \times Et) + e(P \times Et) + u, \quad \textbf{(1)}$$

where Dt is the democracy index at time t; $Dt - 1$ is the democracy index from the year before; Et is energy consumption per capita (logged to the base 10) at time t; $(M \times Et)$ is the dummy variable for semiperphery status multiplied by Et; $(P \times Et)$ is the dummy variable for periphery status multiplied by Et; a, b, c, d, and e are parameters to be estimated; and u is an error term.

MODEL ESTIMATION

When OLS is applied to a pool of cross-sectional and time-series data, the violation of classical regression assumptions is highly likely. In fact, Stimson claims that autocorrelation and heteroskedasticity are virtually "inherent" in pooled data. (1985, 919). (Unfortunately, Arat [1988] relied exclusively on OLS in her pooled analysis; Gonick and Rosh [1988] ignore the autocorrelation problem.) From the time-series component, we must anticipate the former problem, from the cross-section component, the latter. Therefore, our estimation utilizes what Kmenta (assuming the same autocorrelation per unit), calls a "cross-sectionally heteroskedastic and timewise autoregressive model" (1986, 618), appropriately modified to cope with the special problem of having a lagged dependent variable, $Dt - 1$, as a predictor. (This condition also renders the estimates of another common technique, least squares dummy variables (LSDV), inconsistent [Hsiao 1986, 72]).[1] We shall treat the question of the lagged dependent variable, then go on to assess heteroskedasticity. Finally, we shall develop the case for the preferred estimator, GLS–ARMA.

Because $Dt - 1$ is on the right-hand side of the equation, usual methods (e.g., the Durbin–Watson test) for diagnosing autocorrelation are misleading. However, when the appropriate *h*- and *m*-tests were applied, we found the OLS estimates exhibited significant first-order autocorrelation (Ostrom 1990, 66). Therefore, we turned first to an instrumental variables (IV) approach. These results provided an estimate of this first-order autocorrelation ($p = .90$).[2] Furthermore, these IV–OLS estimates revealed highly significant cross-sectional heteroskedasticity, LM = 1,776.86 (on this Breusch-Pagan LaGrange Multiplier statistic, see Greene 1990, 465–69).

Thus, the variables were adjusted for this first-order autocorrelation, and heteroskedasticity was corrected (with the "force homoscedastic model" option in Microcrunch). The estimation procedure was GLS–ARMA (to allow a final check on any residual autocorrelation). In addition, we evaluated the possibility of mutual unit correlation, concluding that it was not a serious threat.[3]

These GLS–ARMA estimates appear below. The residuals from this estimation were scrutinized in an examination of the pooled diagnostics. Following

Stimson, we paid special attention to the summary value of unit residuals, the ratio of residual variance, and any leftover nonstationarity in the autocorrelation function (1985, 939). According to these three diagnostic criteria, GLS–ARMA does edge out rival estimators: IV-OLS, GLS–error components (GLSE), or LSDV.[4]

$$D't = \underset{\substack{(.06)\\ [5.89]}}{.35^*} + \underset{\substack{(.02)\\ [3.97]}}{.09^*}D't - 1 + \underset{\substack{(.22)\\ [11.43]}}{2.49^*}E't$$

$$- \underset{\substack{(.19)\\ [7.15]}}{1.33^*}(M \times Et)' - \underset{\substack{(.18)\\ [8.48]}}{1.54^*}(P \times Et)', \quad \textbf{(2)}$$

with pseudo-R = .71 and N = 2096, where all variables are defined as in equation 1 but transformed (as indicated by the ′) on the basis of the instrumental variable first-order autocorrelation estimate, then submitted to GLS–ARMA estimation, under the "force homoscedastic model" in Microcrunch. The figures in parentheses are standard errors, * means statistical significance at .01 ($t > 2.58$), the figures in brackets are absolute t-ratios, and pseudo-R is the correlation between the observed Dt and the Dt predicted from using original equation 1 variables with equation 2 coefficients. Sources for the democracy and world-system data are described in the Appendix B. The energy consumption data were gathered from United Nations publications.

On the basis of these results, economic development has a highly significant impact on democratic performance. Moreover, when that impact is conceived of as interactive, rather than additive, the interaction formulation has the edge.[5] Economic development matters most for nations in the core, where c = 2.49; it still matters, but about half as much, in the semiperiphery (2.49 − 1.33 = 1.16 net economic effect); and for nations in the periphery, the economic effect is just a bit less (2.49 − 1.54 = .95). Taken together, economic factors, both international and domestic, appear decisive in shaping a nation's democratic future. Still, there is not economic determinism. Other important social forces, captured at least partially by past values of the democracy index itself, exercise an effect. The model, by blending economic with social explanation, manages a good fit to the data, as indicated by the pseudo-R. But to expand the implications of the model, three methodological concerns need to be addressed: measurement error, ceiling effects, and causality.

METHODOLOGICAL CONCERNS

An assumption underlying the estimates of equation 2 (as well as the estimates from other quantitative democracy studies) is that the variables are measured without error. However, common measures of democracy have considerable error. As mentioned, Bollen (1993) demonstrated substantial error in the Banks measures. He also explored Gastil measures (essentially the two components of our study), assigning political rights a validity rating of 93% and civil liberties a validity rating of 78%. Encouragingly, these validity scores have a higher average (86%) than the Banks indicators (average validity = 62%). However, they are not error-free. According to Bollen, the two Gastil components have systematic error, respectively, of 7% and 16%, induced by the Gastil rating method itself.

The question is whether this "methods factor" (as Bollen calls it), even though relatively small, seriously biases our inference about economic effects. Some critics of Gastil have argued, on the basis of their impressions, that the measures he provided Freedom House have a conservative bias. For example, nations that are staunch American allies are alleged to get more democratic ratings than they deserve (Hartman and Hsiao 1988). Using 1980 data, Bollen (1993) assesses the methods bias in the Gastil measure nation by nation, where a high standardized score implies that the judge is too favorable, a low score, not favorable enough, and zero, a neutral judgment. If the Gastil measure has a conservative bias, then that could bias the economic development parameter estimate. To test this possibility, we sequentially reestimated equation 2, progressively eliminating nations with positive scores (i.e., first greater than 2 standard deviations above the neutral value of 0, then greater than 1.5, then greater than 1.0). The economic development coefficient remained statistically significant, its magnitude virtually unchanged: 2.51, 2.52, and 2.52, respectively. Thus, there is no evidence that conservative bias in the Gastil measure, to the extent it exists, has biased the economic development coefficient. (We repeated the same test with the negative scores, in case of a possible liberal bias, and again concluded that the economic development coefficient was unaffected.)

Of course, methods factor error is not exclusively a question of conservative or liberal bias. Other issues, such as the occasional adjustment of democracy scores to reflect poor economic conditions, have been raised *Freedom in the World* 1978:13). What would be useful is some general way of comparing our Gastil measure to a "true" measure of democracy. Fortunately, Bollen has constructed an index for 1980 that is highly correlated with the latent variable of democracy and virtually uncorrelated with the distorting methods factors (1993, app.). These uncontaminated 1980 index scores may be rather close to "true" democracy scores. While available only for 1980, they nevertheless provide invaluable baseline data. We find that this 1980 Bollen democracy index correlates .93 with our Gastil democracy measure for the same year. (All nations were measured on both, except for one, giving N = 130.) This suggests that very little error remains in the Gastil index as constructed, since it correlates almost perfectly with Bollen's virtually error-free democracy measure. In other words, the

Gastil index, as assembled here, does an excellent job of tapping the underlying real variable of democracy.

While the Gastil index has considerable validity, it is not flawless. Like other leading democracy scales, it has limited refinement. Nations can score only so high and no higher. For instance, in the widely used 1965 political democracy index, also created by Bollen, 27 out of the 123 nations sampled score at the top, 90–100 (1980, 387–88). Of course, these scores reflect the scale "ceiling," not the attainment of ideal democracy. Our Gastil measure allows a bit more variation here, with only 20 out of the 131 nations attaining a top score of 13 or 14 across the time period. Moreover, because of the time-series component of the design, nations obviously can attain more than one score. Overall, the added variance on the dependent variable afforded by this pooled design makes for more efficient statistical inference.

A last methodological concern is over causality, an issue that plagues this literature. Does economic development "cause" democracy? Or does democracy really "cause" economic development? The cross-sectional work has had to assume causality runs from economics to democracy, while the limited-time-series work has not fully utilized the testing opportunities that temporal observation allows. Fortunately, Granger (1969, 1988) offers some straightforward causality tests applicable to time series. Accordingly, if X causes Y, then past values of X should aid in the prediction of Y (even after controlling for past values of Y). Further, Y should not aid in the prediction of X.

To carry out the test (here extended to these pooled time-series data), we first established the optimal number of lags for the independent variables, using the final prediction error test (see Mahdavi and Sohrabian 1991, 44–45). With regard to predicting democracy (Y), optimal lagged variables turn out to be $Dt - 1$ and $Et - 1$. With regard to predicting economics (X), the optimal lagged variables are more elaborate, $Et - 1 \ldots Et - 6, Dt - 1$. The block F-tests on the unrestricted versus restricted OLS equations on the pool are, for democracy, $F\ (1,2224) = 18.18$ (significant at .05), and for economics, $F\ (1,1572) = .00$ (not significant at .05).

According to the first test, the lagged economic development variable is a significant predictor of democracy even after controlling for past values on democracy. Therefore, we reject the hypothesis that economic development does not cause democracy. Following the second test, the lagged democracy variable is not a significant predictor of economic development after controlling on past values of economic development. Therefore, we accept the hypothesis that democracy does not cause economic development. These tests allow, for the first time, a rather firm conclusion on this causal relationship: economic development causes (or at least Granger-causes) democracy, but democracy does not cause economic development. (These Granger results seem especially robust, even surviving controls on world-system position.)[6]

CONCLUSION

The economic development thesis, long a staple of comparative democracy studies, has recently come under challenge. The pooled-time-series studies of Arat (1988) and Gonick and Rosh (1988), while in some ways the most advanced quantitative pieces in this critical literature, appear to be characterized by five flaws: (1) they measure democracy with error-laden data (2) the data do not extend past the 1970s, (3) the models are misspecified, in particular with regard to world-system-position interaction effects, (4) estimation procedures are not dynamic and fail to utilize more efficient estimation techniques such as GLS–ARMA, and (5) causality tests are not administered. In response, we have analyzed a very large sample of nations (131) over a long period of time (1972–89), employing a relatively neglected data set on democracy. After refining the model specification, a pooled design was applied, with final parameter estimates provided by the GLS–ARMA technique. The results were demonstrated to be methodologically robust. In particular, the Gastil democracy index has considerable measurement validity and promises to be increasingly used in future work.

We established that the causal arrow most probably runs from economic development to democracy, rather than vice versa. Further, that effect is highly significant statistically. Substantively, the economic development coefficient of equation 2 implies that for every tenfold increase in per capita energy consumption, the nation could expect about a two-and-a-half-point rise on the democracy scale. How important an effect is this? A standard deviation increase in per capita energy consumption (logged) is predicted to yield, on average, a .48 standard deviation change in the democracy score. By this measure, the economic impact is not small, and as effects cumulate over time, the impact can grow. A simple measure of importance comes from correlating average (over nations) world energy consumption with the average (over nations) world democracy score over time (1972–89), $r = .82$.

On balance, it is clear that economic development substantially improves a nation's democratic prospects. However, the full magnitude of that effect depends on the location of the nation in the world system. As the nation moves from the core, to the semiperphery, to the periphery, the effect diminishes. Even in the periphery, however, the effect remains statistically and substantively significant. Thus, around the world, economic development works to foster democracy. Indeed, our Granger results indicate that the relationship works in that direction but not the other. To the extent that this finding holds for nations currently in democratic transition, the implication is that democratic reform by itself cannot be counted on to bring about the needed economic development. However, this is no counsel to dictatorship. Just as clearly, we found that democracy, while not apparently a direct cause of economic development, certainly does it no harm.

Moreover, as the lag pattern of the structural model shows, past democratic performance breeds future democratic performance. Democracy, then, can be furthered for its own sake, without sacrificing economic development.

APPENDIX A: THE GASTIL DEMOCRACY DATA

The democracy data are compiled from Raymond D. Gastil's *Freedom in the World: Political Rights and Civil Liberties* 1989: 50–61 for the years 1972–88 and 1990: 312–13 for the year 1989. (Freedom House assumed responsibility for the scoring after the 1989 issue.) Gastil ranks each nation on separate 7-point scales for "political rights" and "civil liberties." Before assigning a score, Gastil carries out "library research," including the consultation of the *New York Times, Christian Science Monitor,* Banks's *Political Handbook of the World,* regional journals, international press association publications, Amnesty International, State Department reports, and specific country publications (*Freedom in the World* 1989:26; see also Bollen, 1986: 583–84). The categories for each of his checklists follows. (*Freedom in the World* 1989:9). Note that he avoids a simple one-to-one correspondence of a checklist item and a scale point, in arriving at his overall score.

Checklist for Political Rights

1. Chief authority recently elected by a meaningful process
2. Legislature recently elected by a meaningful process
3. Fair election laws, campaigning opportunity
4. Fair reflection of voter preference in distribution of power
5. Multiple political parties
6. Recent shifts in power through elections
7. Significant opposition vote
8. Freedom from military or foreign control
9. Major groups allowed reasonable self-determination
10. Decentralized political power
11. Informal consensus, de facto opposition power

Checklist for Civil Liberties

1. Media/literature free of political censorship
2. Open public discussion
3. Freedom of assembly and demonstrations
4. Freedom of political organization
5. Nondiscriminatory rule of law in politically relevant cases
6. Free from unjustified political terror or imprisonment
7. Free trade unions or peasant organizations
8. Free businesses or cooperatives
9. Free professional or other private organizations
10. Free religious institutions
11. Personal social rights

APPENDIX B: THE SAMPLE OF NATIONS

Nations are listed with their range of democracy scores constructed as described, from the material in Appendix A (2–14 being the widest possible range) and their world-system Position (*c* = core, *m* = semiperiphery, *p* = periphery). To construct the world-system-position dummies, we combined the ratings in nine studies, following a "panel of experts" strategy (Arrighi and Drangel 1986; Bollen 1983; Chase-Dunn 1983; Chirot 1977; Frank 1969; Gonick and Rosh 1988; Nemeth and Smith 1985; Smith and White 1992; Snyder and Kick 1979). There was high agreement among the judgments. However, when discrepancies occurred, more weight was give to the more recent evaluations. As can be seen, the classification has high "face validity"; and when we experimented with alternate classifications, the results did not change substantively.

Afghanistan 2–7 p, Albania 2–2 p, Algeria 3–6 p, Argentina 4–13 m, Australia 14–14 c, Austria 14–14 c, Bahamas 11–13 p, Bahrain 5–8 p, Bangladesh 4–10 p, Barbados 13–14 p, Belgium 14–14 c, Benin 2–4 p, Bolivia 4–11 p, Brazil 6–12 m, Bulgaria 2–4 m, Burkina Faso 3–11 p, Burma 2–4 p, Burundi 2–4 p, Cambodia 2–5 p, Cameroon 3–6 p, Canada 14–14 c, Central African Republic 2–4 p, Chad 2–4 p, Chile 4–13 m, China 2–4 p, Colombia 9–12 m, Congo 2–5 p, Costa Rica 14–14 p, Cote d'Ivoire 4–6 p, Cuba 2–4 p, Czechoslovakia 2–4 m, Denmark 14–14 c, Dominican Republic 9–13 p, Ecuador 4–12 p, Egypt 4–8 p, El Salvador 6–11 p, Equatorial Guinea 2–4 p, Ethiopia 2–5 p, Fiji 5–12 p, Finland 12–14 m, France 13–14 c, Gabon 4–5 p, Ghana 3–11 p, Greece 4–13 m, Guatemala 4–12 p, Guinea 2–4 p, Guyana 6–12 p, Haiti 3–7 p, Honduras 6–11 p, Hungary 4–9 m, Iceland 14–14 m, India 9–12 m, Indonesia 4–6 p, Iran 4–6 m, Iraq 2–3 m, Ireland 13–14 m, Israel 11–12 m, Italy 12–14 c, Jamaica 11–13 p, Japan 13–14 c, Jordan 4–6 p, Kenya 4–7 p, Kuwait 5–9 m, Laos 2–6 p, Lebanon 5–12 p, Liberia 4–7 p, Libya 2–4 s, Luxembourg 13–14 c, Madagascar 4–8 p, Malawi 3–3 p, Malaysia 5–11 m, Mali 2–4 p, Malta 10–14 p, Mauritania 3–5 p, Mauritius 10–12 p, Mexico 8–9 m, Mongolia 2–2 p, Morocco 6–9 p, Nauru 12–13 p, Nepal 5–9 p, Netherlands 14–14 c, New Zealand 14–14 c, Nicaragua 4–9 p, Niger 3–4 p, Nigeria 4–11 p, North Korea 2–2 p, North Yemen 4–8 p, Norway 14–14 c, Oman 3–4 p, Pakistan 4–10 p, Panama 3–9 p, Paraguay 4–9 p, Peru 4–11 p, Philippines 6–12 p, Poland 4–9 m, Portugal 5–13 m, Qatar 4–6 m, Romania 2–3 m, Rwanda 3–4 p, Saudi Arabia 3–4 m, Senagal 4–9 p, Sierra Leone 5–7 p, Singapore 6–8 m, Somalia 2–3 p, South Africa 5–7 m, South Korea 5–11 m, Spain 5–14 m, Sri Lanka 7–12 p, Sudan 2–7 p, Sweden 13–14 c, Switzerland 14–14 c, Syria 2–5 p, Tanzania 4–4 p, Thailand 4–11 p, Togo 2–4 p, Tonga 8–10 p, Trinidad 11–14 p, Tunisia 5–8 p, Turkey 6–11 m, Uganda 2–7 p, United Kingdom 14–14 c, United Arab Emirates 4–6 m, Uruguay 4–13 p, United States 14–14 c, USSR 2–5 m, Venezuela

12–13 m, Yugoslavia 4–7 m, Zaire 2–4 p, Zambia 5–7 p, Zimbabwe 5–8 p.

Notes

We express special appreciation to B. Dan Wood for his advice on estimation for this pooled time series design.

1. The LSDV technique is also less preferred here because it is a "fixed effects" model, which implies that the observations cannot be treated as a random sample from a larger population. In contrast, these data compose a large, apparently representative, sample from a population (of nations and of time). Thus, country dummies should not enter our analysis wholesale. However, we do include selective introduction of geographic dummies (i.e., the periphery, semiperiphery variables). These variables, in their interaction form, explicitly allow for slope heterogeneity of economic effects across nations but do so in a statistically efficient and theoretically defined way. (To confirm our economic effects results, another treatment for differing slopes or intercepts across nations—the random effects model of GLS-error components—was eventually applied. See n. 4.)

2.First, OLS was applied to the pool of variables in equation 1. The Durbin's $h = 7.87$ indicated the need to reject at the .05 level the hypothesis of no first-order autocorrelation. The *m* test also showed significant first-order autocorrelation but no significant second-order autocorrelation. Finally, more general Breusch–Godfrey testing revealed this significant *AR*(1) process but uncovered no other processes of any order. To estimate this first-order autocorrelation, we used an instrumental variables approach (see Ostrom 1990, 65–71). An instrumental variable, $Dt - 1''$, was constructed (from the exogenous variables of equation 1, lagged to $t - 1$). (The instrument appears a good proxy, $Dt - 1''$ correlating .64 with $Dt - 1$, for which it is a stand-in.) This $Dt - 1''$ was substituted for $Dt - 1$ in equation 1, and OLS was applied to achieve consistent estimates of the parameters and the residuals. Using these residuals, we arrived at an estimated pooled *rho* (the first-order autocorrelation) of .90.

Having thus obtained a desirable estimate of the troublesome first-order autocorrelation, we moved on to correct the original equation for it. Following a Cochrane–Orcutt-type procedure, each original variable of equation 1 was transformed (e.g., $[Dt - 1] - [.90Dt - 2] = D't - 1$). These transformed variables were then submitted to the comparative pooled estimations (see n. 4).

3. Besides homoskedasticity and no autocorrelation, the assumption of the mutual independence of the units can also be a concern with analysis of pooled data. Here we are dealing with a sample of independent nations, rather than a population of more homogeneous geographic units like the American states (Kmenta 1986, 622–25). Therefore, we did not suspect a mutual unit correlation problem. Nevertheless, we attempted to correct for it, utilizing SHAZAM but were initially thwarted by the very high correlation between the error vectors of certain nations. One solution—exploration of the *phi* matrix to identify the offending high correlations—poses its own set of problems, involving a search through about 8,500 correlations (from our 131 × 131 country matrix). Another solution—eliminating countries by trial and error and reestimating—not only raises practical difficulties but would call into question the integrity of the remaining sample of nations. In the end, we chose to stick with our GLS–ARMA estimates, which have been corrected for autocorrelation and heteroskedasticity. We are quite comfortable with this because, in general, a correction for mutual unit correlation increases efficiency and makes for higher levels of statistical significance. The reported GLS–ARMA results of equation 2, then, can actually be viewed as a conservative test.

4. For purposes of statistical comparison, OLS, LSDV and GLSE were applied to the variables of equation 1, corrected for the autocorrelation described in n. 2. (Recall that Arat [1988] relied on OLS, Gonick and Rosh [1988] on GLSE estimation.) The OLS results indicated significant autocorrelation remaining, according to the autocorrelation function (back five lags). The autocorrelation function of the LSDV estimator also revealed a slight amount of nonstationarity remaining, with a statistically significant first-order autocorrelation estimate of .06. GLSE makes no correction for serial correlation (or heteroskedasticity) but might be considered more efficient than LSDV for taking into account any intercept differences (which OLS ignores). GLSE in fact yielded a parameter estimate (of 2.54) for *Et*, almost identical in value to that in equation 2. However, it yielded a standard error of .30, clearly indicating the efficiency gains of the GLS–ARMA (heteroskedasticity-corrected) estimate of equation 2, with its lower standard error of .22.

Further, other residual diagnostics for these GLS–ARMA results are favorable. With GLS–ARMA, the standard deviation of the residual means is only .34, compared to .37 for GLSE. (The residual variance ratios show essentially the same performance level with, for example, only 9 of 131 scores exceeding 3.0 in GLSE or GLS–ARMA.) These residual departures seem small and bolster our confidence in the GLS–ARMA model estimation and specification. (A healthy pattern of residual diagnostics from a GLS–ARMA analysis led Stimson to a similar conclusion regarding his party issue polarization model [1985, 943–44, tbl. 7].) However, pooled diagnostics provide no sharp boundaries for deciding "right" and "wrong." As Stimson goes on to note, at some point it becomes "a judgment call" (p. 943). We experimented with treating the few departing cases as outliers, either eliminating them from the estimation equation or introducing dummy independent variables. We could not improve on the rather robust diagnosis already obtained with the GLS–ARMA estimates of equation 2.

5. Ideally, we would have a fully specified model, including the additive terms for world-system position (*M* and *P*) in equation 2 along with the interaction terms. We estimated this fully specified model, but severe collinearity (e.g., *Et* regressed on the other independent variables yields an R-squared = .994) rendered the coefficients nonsensical. Because this problem sometimes occurs in the presence of interaction terms, we applied the possible solution of centering the variables, but the results failed to improve (Jaccard, Turrisi, and Wan 1990, 31). As an alternative, we estimated a main-effects model, for comparison to the interaction effects model of equation 2. Using the same variable definitions, statistics, corrections, and estimation methods (GLS–ARMA, "force homoscedastic model" option) as in eq. 2, the results are as follows:

$$\begin{array}{lllll} D't = .97^* + & .08^*D't - 1 + & .89^*E't - & 4.86^*M' - & 6.02^*P', \quad (3) \\ (.10) & (.02) & (.21) & (.69) & (.68) \\ [9.54] & [3.80] & [4.29] & [7.06] & [8.80] \end{array}$$

with pseudo-$R = .70$ and $N = 2{,}096$.

In comparing these results to equation 2, we observe that the coefficient for the key economics variable, *Et*, is much less significant than under the interation specification. To appreciate this, first use the standard errors to construct confidence intervals (95%, two-tailed) around the *Et* coefficient in each equation. For equation 2, this confidence interval equals 2.49 +/− (1.96 × .22) = 2.06–2.92; for equation 3, this confidence interval equals .89 +/− (1.96 × .21) = .48–1.30. One observes that for the interaction specification of equation 2, the lower bound of the confidence interval is much farther from zero, giving much more confidence in rejecting the null hypothesis. The greater statistical security of the *Et* coefficient in the original interaction specification (equation 2) is summarized by comparison of the *t*-ratios, 11.43 and 4.29, respectively. Further, as the other *t*-ratios show, the precision of the other coefficients is not lessened.

6. The Granger approach, by only allowing an *X* to account for *Y* after past values of *Y* itself have accounted for all they can is a very conservative test. Hence, the survival of economic effects in such a test gives us some confidence in their existence. Because its purpose is to offer a demanding "test for causality," rather than to estimate a structural model, comparisons of Granger results to structural model estimates such as those of equation 2 can be misleading. However, it is

possible that even strong Granger findings, such as these, are spurious because of the operation of a third variable (Pindyck and Rubinfeld 1991, 217). The obvious "third variable" candidate is world-system position. Therefore, as a further test, we included the world-system interaction terms in the unrestricted Granger regression:

$$\begin{array}{l} Dt = .02 + .96Dt - 1 + .14Et - 1 - .03(M \times Et - 1) \\ \quad (.09)\ (.01) \qquad (.03) \qquad (.02) \\ \quad [.22][160.35] \quad [4.13] \quad [1.63] \\ \qquad\qquad\qquad\qquad - .05(P \times Et - 1), \quad \mathbf{(4)} \\ \qquad\qquad\qquad\qquad (.02) \\ \qquad\qquad\qquad\qquad [2.04] \end{array}$$

with adjusted $R^2 = .953$, $N = 2{,}227$, and Box-Ljung Q (distrb. chi-squared, 5 df) = 5.77, where the variables are defined and measured as in equation 1. The figures in parentheses are standard errors, the figures in brackets are absolute t-ratios, adjusted R^2 is the adjusted coefficient of multiple determination, and the Box-Ljung Q is a measure of autocorrelation.

These results make clear how demanding the Granger-test is. We observe that if we impose no theoretical structure on the model and operate by statistical brute force, past values of democracy appear almost totally predictive of democracy. Nevertheless, even under this extreme (and unrealistic) stricture, economic development manages statistically significant main effects, as well as statistically significant world-system interaction effects. The t-ratios are helpful here. For a one-tailed test, significance at .05 requires $t > 1.64$, and significance at .10 requires $t > 1.28$. Also, the block F-test is still significant at .05, F (3,2222) = 11.22 (a number slightly lower than the original because of different degrees of freedom). Again, we find that economic development is a determinant of democracy, irrespective of world-system position.

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THE DETERMINANTS OF INDUSTRY POLITICAL ACTIVITY, 1978–1986

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While the allocation of interest group monies to specific politicians has been extensively studied, little is known about the factors that determine of the overall level of political activity across groups. We study total contributions by corporate political action committees at the industry level. We create a large data set on industry political activity, covering 124 industries across five election cycles from 1978 to 1986 and sketch out a simple benefit–cost model to predict total corporate PAC contributions in each industry. The few previous studies of this phenomenon use relatively small samples and employ statistical techniques that are either biased or impose untested restrictions. The selectivity-corrected regression technique used here solves these problems. We find that industries with greater potential benefits from government assistance contribute systematically more but that the ability to realize these benefits is constrained by collective action problems facing firms in each industry.

Campaign contributions made by corporate political action committees (PACs) are increasingly important—and controversial—in U.S. elections. "Influence purchasing" is often alleged by pundits, and campaign finance reform is continually debated in Congress. In the academy, social scientists have analyzed the allocation of interest group monies and have looked for possible impacts on legislation. Few, however, have tried to explain the total contributions of economic interest groups. We shall analyze variations in contributions across industries and over time with an empirical model based on organizationally constrained profit-maximizing behavior. We assume that corporate PAC contributions in an industry are raised to enhance industry profits subject to the constraints of organization costs and free-riding incentives. Our empirical work covers 124 industries over five election cycles from 1978 through 1986.

Both the costs and benefits of political activity vary across industries. We argue that the benefits of political action are determined by direct contact with the government as a regulator or purchaser of industry output, by government's ability to ameliorate adverse market conditions, and by the industry's ability to solve collective action problems without government assistance. The costs of industry political action arise mainly from collective action problems, because effective political activity often requires concerted action by the constituent firms in each industry.

We also make a methodological contribution. Previous studies use statistical techniques that suffer from sample selectivity bias or else force the probability of acting politically to be explained by the same set of coefficients as is the amount of action taken. We explain and employ a technique that accounts for both problems. The results are consistent with the theses that industries follow investment-oriented goals in deciding the amount of political action to undertake, and that collective action problems are important constraints on industry political activity. We can explain between 60% and 80% of the variation in contributions in our sample.

First, we review existing work on corporate political activity, particularly papers examining patterns of aggregate PAC spending by industry. Then we present an organization-cost-constrained industry-profit-maximizing model, based on a comparison between an idealized zero-organization-cost industry and the more realistic case where organizing collective action is difficult. The independent variables used in the analysis are also discussed and defined. Next, we discuss the dependent variable and total industry contributions and explain our statistical method. Then the results are presented, followed by a discussion of industry structure and political activity. To make clear how our results can explain different contribution patterns, we give examples using five actual industries.

PREVIOUS WORK ON CORPORATE POLITICAL ACTIVITY

Most work on PACs examines where contributions go and what they accomplish, not where the money comes from. Gopoian (1984), Poole and Romer (1985), Snyder (1990), and Stratmann (1992) all explicitly study the allocation patterns of PACs. Grier and Munger (1991, 1993) find that corporate, union, and trade association PACs target incumbents who win by moderate margins, have voted sympathetically, and (at least in the U.S. House) have seats on committees with jurisdiction over the PACs area of interest. Studies of the *effects* of contributions show more mixed results (e.g., Evans 1988; Grenzke 1989; Hall and Wayman 1990; Langbein 1986; Salamon and

Siegfried 1977; Stratmann 1991; Wright 1989, 1990). Research on specific policies finds some measurable effects. Work on broader measures of influence (e.g., general voting patterns and behavior) finds negligible impacts.[1]

Turning to the sources of money, Andres (1985), Masters and Keim (1985), Grier, Munger, and Roberts (1991), Humphries (1991), and McKeown (1994) all study a binary choice: Does a corporation have a PAC?[2] This question is important, but if we are really to understand the pattern of political action of industries we must focus on the amounts that industries contribute as a collection of firms. Unfortunately, the work on total contributions (our dependent variable of interest) has focused on industry structure (particularly concentration) rather than offering a general model.[3] Pittman (1976) argues that more contributions will be generated by concentrated industries (i.e., a few large firms). Esty and Caves (1982) and Zardkoohi (1985) find ambiguous effects for industry structure. None of these models has worked from an overarching conception of the goals of the firm and the industry and how these goals might be realized through political action.

The data that previous researchers use also vary widely. Pittman (1976) uses a Common Cause data set of large individual contributions to Nixon's reelection campaign, aggregated into industry-level observations. Esty and Caves (1982) aggregate 300 firms from the Fortune 1,000 into 35 industry-level observations. Their dependent variable is total PAC receipts plus estimated lobbying expenditures by industry, summed over the years 1976–78. Zardkoohi's sample is 412 firms with PACs that made contributions in 1980. Boies (1989) examines PAC contributions by the Fortune 500 firms in 1976 and 1980. He begins with 18 independent variables, then considers the 10 that are correlated with contributions in either year.

Though each of these papers makes a useful contribution, they each have problems in research design. The first kind of problem is the choice of a sample to analyze. Pittman, Zardkoohi, and Esty and Caves all consider only industries (in Zardkoohi's case, firms) that are politically active. Though this does allow the use of least squares estimation, using only active PACs is clearly a nonrandom criterion for inclusion in the sample. As Heckman (1976, 1979) shows, if variables, that determine the sample are correlated with variables used to test hypotheses in the sample, ordinary least squares coefficient estimates are biased and inconsistent. It is quite likely that variables influencing whether firms in an industry establish a PAC correlate with variables affecting how much money that PAC then spends. (We show later that this is exactly the case.) Boies's sample includes noncontributing firms, but he chooses his specification with ordinary least squares and then reestimates the "best" model using TOBIT.[4] TOBIT regressions force the model for predicting existence of PACs, and the model predicting the spending of PACs, to have the same set of coefficients (see Appendix A).

The second research design problem is the size of the samples used in these studies. Esty and Caves study 300 firms aggregated into just 35 industry-level observations. Zardkoohi and Boies consider 415 and 500 firms respectively. Pittman does not report the number of industries included in his sample. Further, each paper analyzes only one or two cross sections of contributions. In cases where more than one cross section is examined, regressions are run separately, with no attempt to exploit the time-series property of the data for increased efficiency of coefficient estimates.

We address these research design issues in our empirical work, aggregating thousands of firms into 124 different industries, over five election cycles. We test for, and then reject, the implicit restrictions built into the TOBIT model. The alternate technique first proposed by Heckman (1976) is shown to be a better means of accounting for sample selection.

INVESTMENT IN POLITICAL ACTION BY INDUSTRY

It will be useful to lay out the theoretical perspective that informs the subsequent empirical work.

Theoretical Perspective

The following model captures variations in the likely gains to political action and the costs in achieving those gains, across industries. The maintained hypothesis underlying our work is that in a world of no organization costs, industries would maximize total profits earned by all producers. But as Olson (1965) points out, groups that might form profitably may be prevented from doing so by the free-rider problem. Consequently, the explanatory variables we shall discuss are mostly controls for obstacles to collective action.

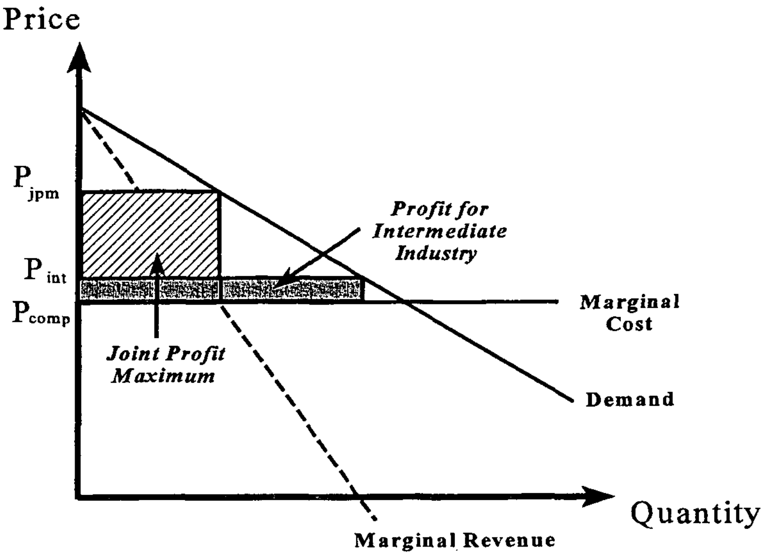
We shall analyze corporate PACs *as if* they were directly controlled by their sponsors in cooperation with other corporations in the industry, to see how much explanatory power this reductionist approach has. Under the assumption that there are no impediments to agreeing on or enforcing collective action, industries would achieve the joint profit maximum. In this idealized setting, we can write the following profit function for industry i:[5]

$$\Pi_i = P_i(Q_i, I_i)Q_i(L_i, K_i) - r_i(I_i, K_i)K_i - w_i(I_i, L_i)L_i - \phi I(\phi)_i, \quad \textbf{(1)}$$

where Π = joint industry profits; P = price of output for whole industry (no cheating), partly politically determined; Q = quantity of output produced, given production technology and amounts of K and L; I = political influence of the industry, an industry-specific function of ϕ;[6] L = quantity of labor used for

FIGURE 1

If Industries Cooperate, Political Action is of Little Value; If Industries Can't Cooperate, Political Action is Costly



production; K = quantity of capital used for production; r = price of capital (interest rate), partly politically determined; and w = price of labor (wages), partly politically determined. The firm invests in all activities with a rate of return greater than the risk-adjusted cost of borrowing. Political activity will become attractive if its net present value is greater than zero. It is worth noting, however, that unlike labor and somewhat like capital investments, political activity may be "lumpy." Though many firms have established PACs, many more have not. As Eismeier and Pollock note, the costs of establishing and running a PAC (including reporting and accounting requirements), are significant, so that the minimum investment required to make contributions is substantial (1988, 101). In the empirical work that follows, we estimate the activity decision first as a PROBIT. We assume that labor and capital markets are segmented and that the industry can affect P, r, and w through government influence.[7] For example, the auto industry can affect P by supporting protectionist legislation, w by gaining power over unions, and r through regulation of financial markets and monetary policy.

We must account for an important additional factor to make the model useful for application to actual corporate political activity. The industries with the lowest costs (in terms of acting collectively) of using political action are also the industries for which political action holds the lowest marginal benefits. Industries that can act as a unit are quite likely able to achieve most benefits of cartelization on their own. As Posner points out: "The demand for regulation . . . is greater among industries for which private cartelization is an unfeasible or very costly alternative—industries that lack high concentration and other characteristics favorable to cartelizing. They lack good substitutes for regulation" (1974, 345). Figure 1 illustrates this trade-off. If the industry can act perfectly collusively, privately, it can charge P_{JPM} and achieve the private, joint profit maximum. Political influence might increase profits further, by protecting the industry's output for competition, increasing government purchases, or making cheaper loans or labor contracts available. But these gains may be smaller than the potential gains of more competitive industries: the cost of increasing profits rises as the industry nears the joint profit maximum (Denzau and Munger 1986; Peltzman 1976).

A purely noncooperative industry, with no private collective agreement on price and output, charges price equal to marginal cost (the flat line in Figure 1) and makes zero economic profits. This industry serves a similar market to—and faces the same input costs as—the joint profit-maximizing industry discussed above, but $\Pi = 0$, because of collective action problems. The benefits to political action for a competitive industry are large, but the costs of overcoming the collective action problem if firms are atomistic are larger still. Finally, the "intermediate" industry charges P_{INT}. Political action is more costly for intermediate industries than for perfectly organized industries, but the benefits are far larger than for privately colluding groups because intermediate industries cannot reach P_{JPM} on their own.

To summarize, we claim that if the marginal costs and benefits of political activity are accounted for, the pattern of contributions can be explained. The null hypothesis against which our theory is tested is that corporate PAC contributions by an industry are explained by idiosyncratic internal imperatives, not external investment-oriented goals. This view is mostly associated with the interview results of Bauer, Pool, and Dexter (1963); Handler and Mulkern (1982);

and the work of Wright (1985) on trade associations. Rejection of the null will not imply that American business is a monolith and acts only on its collective interests, of course. Rather, if the null hypothesis is rejected, it simply means that corporations in industries in our sample respond systematically and predictably to political incentives in choosing their levels of political activity.

Finally, it is useful to distinguish PACs sponsored by corporations and the trade association PACs that represent the same industry. Previous work on trade association PACs (Grenzke 1989; Wright 1985) or work that has mixed trade association and corporate PACs (Wright 1989, 1990) is useful as research on interest groups generally but tells us little about the general pattern of corporate political action. Corporate PAC contributions at the industry level are important in their own right, for at least three reasons.

First, trade associations are subject to the statutory limit of $10,000 per election cycle (primary and general) per candidate, while PACs representing individual corporations in the same industry can make many contributions of up to $10,000. Consequently, trade associations often represent groups with many diffuse members. For example, in 1988, the National Association of Realtors spent $3 million, the American Medical Association $2.7 million, and the Association of Trial Lawyers $1.9 million. Trade associations are less important in industries with a few large firms, however: the "motor vehicles and equipment" industry gave more than $1.8 million, with just the Ford, Chrysler, and General Motors PACs spending $1.2 million. Second, collective action by corporations is a substantive and interesting problem in its own right. Umbrella organizations such as trade associations are one means by which collective action problems can be overcome, but only because the costs of organizing and raising funds are internalized. We address collective action directly, by explaining differences in industries' ability to overcome free-rider problems and induce corporations to act. Finally, government actions often affect the entire industry. The Environmental Protection Agency regulates use of chemicals by dry-cleaning firms, the National Highway Traffic Safety Administration requires all autos to meet safety standards, and an increase in the oil depletion allowance affects the net profits of all oil producers. Firms that can act as an industry will be much more effective at deflecting or diluting unfavorable regulation.[8] Since the pattern of contributions *observed just one firm at a time* could be consistent with either pure cooperation or purely atomistic behavior, we use the *industry* as the unit of analysis.

Definition of *Industry*

The intuitive definition of an industry is just "all firms that produce similar products." Of course, this requires a definition of *similar*. The Census of Manufactures has a range of industry definitions, or standard industrial classifications (SICs), from two-digit (very broad) to five-digit (extremely specific). For example, SIC 20 is "food and kindred products," SIC 202 is "dairy products," and SIC 2024 is "ice cream and frozen desserts." Similarly, SIC 50 is "durable goods–wholesale," 504 is "professional and commercial equipment supply–Wholesale," and 5045 is "computers and software–wholesale." We need to use a definition of industry that includes enough observations that the sample can be analyzed statistically yet ensures that the firms we will call an industry produce similar outputs. To strike a balance between these two concerns, we have chosen the three-digit SIC level as the definition of industry.

The variables we propose to use to explain industry political activity will be defined and discussed. These variables are designed to capture the determinants of variations in political activity across industries, that is, the marginal benefits of political action to the industry, the marginal costs of political action to the industry, and the collective action costs faced by the firms in the industry in organizing politically.

Empirical Model

To specify an empirical model embodying these ideas, we shall consider each of three sets of explanations in turn: direct political influences on demand for the industry's output, political influences on underlying market conditions, and the costs of collective action. A brief description of the variables we use to measure these concepts is given here. (For a complete description of the definitions and sources of the variables, see Appendix B.)

Direct Political Influence on Demand. The federal government can affect the demand for the industry's output directly by being a consumer and indirectly by being a regulator. The size of average sales to the government and whether the industry is subject to *regulation* (a dummy variable, equal to 1 for 17 industries in the sample subject to specific federal regulation) are the most important indicators of direct government impact. Our measure of regulated industry is taken from Pittman (1977). *Average government sales* is the mean (in millions of dollars) amount of sales by firms in each industry and is taken from Bureau of the Census (1985a–d). These variables represent the degree that prices are determined by a political process and may be sensitive to political influence. Consequently, both regulation and government sales should cause higher contributions.

Amelioration of Adverse Market Conditions. As noted, for an industry to profit from favorable regulation, both its input and output markets should be protected from unexpected shocks and from potential competitors. Government can also indirectly affect both the level and variability of demand for the industry's product and competition from imports. Our measure of variability of demand that the industry faces is *standard deviation of industry profits,* based on time-series data on real corporate profits (again, in 1988 dollars) provided by the Bureau of Economic

Analysis at the two-digit SIC level. The variable used is the standard deviation of profits over the four years preceding each election in the sample. We expect this variable, which measures variations in the riskiness of profits across industries, to be positively associated with political activity. The reason is that risky industries may try to get government either to underwrite their losses (as was the case in the bailout of Chrysler and the savings-and-loan industry) or to pursue policies that will support demand. Examples of the latter include price floors on commodities or the extension of "most favored" trade status to nations with many consumers and many large human rights violations. *Import share* is a measure of the industry's exposure to foreign competition, which might be moderated by government intervention. Ceteris paribus, industries with higher import shares will use the political process more heavily, at least up to some critical level of imports. The source for import share is Clark, Kaserman, and Mayo 1990.

If the industry uses specialized labor, political influence on Congress and the National Labor Relations Board can affect wages industry by industry. The federal government also influences the risk-adjusted price of borrowing funds through management of the money supply, but no good industry-specific measures of interest rates are available. Consequently, our measures of input prices are real wage rate and BAA bond rate. *Wage* is the annual inflation-adjusted wage rate for production workers in the industry; the data come from the CITIBASE economic data bank. This variable is available only at the two-digit SIC level and only for industries 20–49. *Bond rate* is available annually and is the average rate charged on BAA-rated bonds as reported by CITIBASE. All else equal, high-wage industries should be more politically active, and all industries should be more politically active in years when interest rates are high.

Collective-Action Problems. Although, for the sake of exposition, we assume that the industry faces no collective-action problems and can achieve the joint profit maximum of a pure cartel, differences in the costs of collective action are the key variables in the choice to seek political influence. There are two general categories of measures to be included: measures of the extent to which the industry has common interests, and measures of the industry's ability to overcome the free-rider problem.

We include two measures of the extent to which the industry has common interests: geographic concentration and diversity of the products that the industry produces and sells. *Geographic concentration* is a measure of whether an industry has good alternatives to contributions as means of gaining political influence (e.g., lobbying, direct appeals to voters). If an industry is located entirely in a single state, its employees are an important voting bloc for legislators in that state when compared to an industry with the same number of employees spread uniformly over all 50 states. We use a Herfindahl index to measure dispersion of industry sales across states; greater geographic concentration produces a higher index.[9] The expected sign for the coefficient on this variable is negative, since votes can substitute for contributions.[10] *Diverse industry* is a dummy variable selecting heterogeneous industries, in terms of number of different products. The variable equals 1.0 for the 30 industries that have more products than two standard deviations above the mean. To the extent that industries with greater product diversity have weaker common interests among firms, we expect contributions to be lower.[11] Both geographic concentration and diversity are adapted from COMPUSTAT.

We account for the ability of the industry to overcome the free-rider problem in three ways: firm size, industry structure, and government reaction to collusive arrangements. The first of these is the most obvious; in Olson's (1965) framework, the key element is the absolute size of benefits to the individual. Since in our analysis the "individual" is the firm, we use *average private sales* (measured in millions of 1988 dollars) as a measure of firm size. The relative cost of raising funds falls with larger firms, at least over a certain size range. Political action committee money has to be raised from employees in sufficient quantities to have some effect; larger firms have more employees and are better able to pay the fixed start-up and accounting costs for establishing a PAC. The data on mean firm size within industries is from COMPUSTAT. The industry structure variable is the four-firm concentration ratio (hereafter, *concentration*). *Concentration* is the proportion of total industry sales accounted for by the four largest firms (the data used here are from the Bureau of the Census 1985a). In more concentrated industries, the smaller number of similar-sized firms simplifies formation and enforcement of agreements over contribution strategies and raise total contributions, at least over some range. As industries become highly concentrated, firms presumably have sufficient market power to earn maximal profits with less and less political help. *Cumulative antitrust indictments* measures the ability of the industry to solve problems of collective action privately without attracting attention from the Justice Department and Federal Trade Commissions, which try to ensure private collective action problems stay unsolved. Other factors equal, antitrust actions make firms more sensitive to the political environment and raise contributions. Our measure comes from Miller 1989 and Miller, Shughart, and Tollison 1990.

The variables described above account for several different effects. Industry structure for example, proxies for both the costs and the benefits of political action. Sales to the government measure both the dependence on political action and the existence of direct ties with specific agencies or departments. Our goal has been not to discriminate among theoretical models of political investment (which do not yet exist) but to specify a simple theoretical model consistent with the empirical model we estimate. Such an approach allows us to test the value of an organiza-

TABLE 1

Corporate PAC Contributions to Congressional Candidates in 124 Industries (Three-Digit SIC), 1978–86

YEAR	TOTAL CONTRIBUTIONS ($)	LARGEST SINGLE-INDUSTRY CONTRIBUTION ($)	INDUSTRIES MAKING NO CONTRIBUTION	AVERAGE NONZERO CONTRIBUTION ($)
1978	8,846,904	1,409,900	57	132,043
1980	15,268,120	1,873,400	41	183,935
1982	17,690,584	2,045,600	39	208,124
1984	20,281,440	2,480,500	39	238,605
1986	24,721,880	2,715,400	40	294,308

Note: All figures are in 1988 dollar equivalents.

tionally constrained investment conception of corporate political action.

DATA AND TECHNIQUE

Our dependent variable is corporate campaign contributions to all U.S. House candidates, taking industries as the unit of observation. These data are available by individual PAC from the Federal Election Commission. We merged individual corporate PAC contributions with firm data from COMPUSTAT for the five election cycles 1978–86 and then aggregated up to the SIC three-digit industry level. The data collection process captures 50%–60% of the corporate PACs (listed separately from the trade association records, there being no overlap between the two) listed in the Federal Election Commission data for each election and about 80% of the dollar amount of contributions. The 124 industries in the sample are mainly in the 200–399 (manufacturing) range along with a few 100-level (agricultural and mining) and 400-level (services) industries. Industries in the sample and their contributions levels are listed in Appendix C.

Table 1 shows the growth of political activity in the 124 industries comprising the sample. Total contributions almost triple in real terms (all dollar figures are given in 1988-dollar-equivalent units) over the sample, from around $8.8 million in 1978 to almost $25 million in 1986. Both the intensity of participation by active industries, and the number of industries making contributions, grows significantly. The largest total contribution by a single industry almost doubles, from $1.4 million in 1978 to $2.7 million in 1986, and the average nonzero contribution rises from $132,000 to $294,000. Further, the number of industries making no contributions falls from 57 to 40.

The values of the variables are calculated for two different samples: the full 124-industry sample and the subsample of 110 industries where complete data for the full model are available. The two samples appear generally similar (see Appendix B for summary statistics and precise variable definitions), though industries with the largest average government sales, most antitrust indictments, and most variable profit streams are excluded in the smaller sample, reducing the sample variation of these regressors.

The methodological problem is that the dependent variable is limited below, at zero. Industries where it is not profitable to make political contributions or where the collective-action problem cannot be solved make no contributions. Zero contributions occur in 216 (35%) of the 620 observations in the full 124-industry-and-five-election sample. Such a probability mass at a single point implies biased and inconsistent ordinary least squares estimates. A possible solution is to throw out the zero-contribution observations and then estimate an equation on the rest of the sample with OLS. This approach eliminates the mass-point problem but introduces sample selectivity bias, again yielding biased and inconsistent coefficient estimates.

We can write the HECKIT (Heckman 1976, 1979) model as follows:

$$z_i^* = X_{1i}\beta_1 + \epsilon_{1i} \quad (2)$$

$$y_i = X_{2i}\beta_2 + \epsilon_{2i}. \quad (3)$$

Equation 2 is the sample selection equation where z_i^* is an unobserved index of the propensity to undertake political action. We do not observe this index but do observe an indicator variable z_i that equals 1 if $z_i >$ 0 and equals 0 otherwise (in our case, $z_i = 1$ if any firms in the industry have active PACs). Equation 3 is the contributions equation where y_i is observed only when $z_i = 1$. If $X_1 = X_2$, $B_1 = B_2$ and $e_1 = e_2$, then the model is identical to the TOBIT model. On the other hand, if these conditions do not hold, then TOBIT imposes invalid restrictions on the data. We show in Appendix A that the TOBIT restrictions are rejected in our sample. Consequently, we use the HECKIT estimator to allow B_1 to differ from B_2, while continuing to assume $X_1 = X_2$. We assume that e_1 and e_2 are bivariate normal random variables with correlation coefficient ρ.

The basic problem with just estimating equation 3 with OLS is that the expected value of the error term is both nonzero and correlated with the independent variables; that is,

$$E[y_i \mid z_i = 1] = X_{2i}\, B_2 + E[e_{2i} \mid z_i = 1]. \quad (4)$$

TABLE 2

Determinants of Corporate PAC Contributions to House Candidates by 124 Industries, 1978–86

	EQ. 1: PROBIT		EQ. 2: HECKMAN[a]	
VARIABLE	COEF.	S.E.	COEF.	S.E.
Constant	−1.167	.45*	−241,199	103,076*
Trend	.117	.47*	40,915	9,741**
Private Sales	.227	.03**	21,198	2,924**
(Private sales)^2	−.001	.0002**	−64	16**
Government sales	.708	.35*	132,302	15,402**
(Government sales)^2	−.012	.01	−2,320	358**
Regulated industry	.260	.26	167,292	39,831**
Diverse industry	−1.172	.38**	−394,612	92,199**
Concentration ratio	.037	.017*	3,549	3,697
(CR)^2	−.00053	.00021	−38	38
Cumulative antitrust	.010	.017	9,316	3,682*
S.D. of profit	3.105	.61**	222,127	71,195**
Geog. spread	−.002	.000034	−390	89**
IMR	—	—	182,087	60,294**
Dependent variable	0/1		dollars	
Sample	620		404	
Log-likelihood/R^2	−251		.601	

[a] Selectivity-bias-corrected OLS with corrected standard errors (Heckman). Data are on 124 three-digit industries for five elections expressed in 1988 dollars; 404 nonzero observations and 216 at 0. To allow more significant digits, the sales data used in these regressions are in one hundred's of millions of 1988 dollars.

*$p \leq .05$.

**$p \leq .01$.

Greene gives the expectation of the error term in equation 4 as

$$E[e_{2i} \mid z_i = 1] = \rho\sigma_y\{f(Z_i)/1 - F(Z_i)\},$$

where $Z_i = (X_{1i}B_1/\sigma_{e1})$ and f and F are the normal and cumulative normal densities, respectively (1993, theorem 22.4).

Intuitively, accounting for the sample selection process generates another regressor, $f(Z_i)/1 - F(Z_i)$, which needs to be incorporated into the contributions equation. This variable is generally called the inverse Mills ratio (IMR), or the Hazard rate. The HECKIT technique first estimates a PROBIT model on equation 2 (using the (0, 1) indicator variable z) to calculate an estimate of the IMR for each data point. Then this equation is estimated using OLS:

$$y_i = X_{2i}\, B_2 + g\, \hat{\text{IMR}} + v_{2i}.$$

HECKIT provides consistent estimates of B_2, though the OLS standard errors are biased. We compute the consistent standard errors as derived by Greene (1993, 713).

RESULTS

The sample is a pooled time-series cross section, so our first task is to find out whether the time-series pooling assumption is valid. In all the regressions reported here, there is a trend variable (trend = 1 in 1978 and goes up by 1 in each period to 5.0 in the 1986 election) included as a regressor. The variable imposes the restriction that the intercept shifts an equal amount between each election. This restriction can be tested by replacing the trend variable with four dummy variables that let the intercept shift freely and then comparing the fit of the two equations with a likelihood ratio test. In all cases, the appropriateness of the trend variable cannot be rejected, even at the .10 confidence level.

We also test for the stability of the other coefficients in the regression over time using likelihood ratio tests. The hypothesis that a pooled sample is appropriate at the .05 level, once the trend intercept shift is allowed, cannot be rejected. Thus the annual estimates do pool over the years in our sample, and treating these data as a single sample is legitimate.

Equations 1 and 2 carry out the Heckman procedure (Table 2). The PROBIT model from equation 2 provides an estimate of the IMR that is used as an additional regressor in the reported OLS regression. The selectivity-corrected OLS model shows all variables correctly signed and significant at the .01 level except concentration.[12] This regression explains 60% of the variation in contributions across politically active industries. Also, the coefficient on the IMR is positive and significant in equation 3, suggesting that selectivity is a factor in these data. In other words, OLS estimations on this sample would produce biased estimates of the true coefficients. The PROBIT model (equation 1) correctly classifies 80% of the cases in the sample. The misclassified cases are split almost evenly between inactive industries predicted to be making contributions (61 cases) and contributing industries predicted to be inactive (64 cases).

Our technique allows a comparison of the effects of

TABLE 3

Adding Import Competition, Interest Rates, and Wage Rates in a 110-Industry Subsample

	EQ. 1: PROBIT		EQ. 2: HECKMAN[a]		EQ. 3: PROBIT		EQ. 4: HECKMAN	
VARIABLE	COEFF.	S.E.	COEFF.	S.E.	COEFF.	S.E.	COEFF.	S.E.
Constant	−1.818	.53**	−326,652	87,574**	−2.078	821*	−700,220	128,954**
Trend	.099	.51	29,002	7,287**	.072	.052	29,187	7,136**
Private sales	.230	.031**	18,786	2,108**	.238	.031**	17,779	1,991**
(Private sales)^2	−.002	.000042**	−90	11.6**	−.001	.00018**	−89	11**
Gvt. sales	1.108	.42**	160,011	40,024**	1.190	.43**	148,410	39,055**
(Gvt. sales)^2	−.231	.12*	28,875	8,134**	−.242	.12*	29,964	7,885**
Regulated industry	.243	.33	137,784	33,606**	.187	.35	103,870	36,318**
Diverse industry	−1.315	.41**	−192,803	65,357**	−1.453	.44**	−168,490	58,913**
Conc. ratio	.067	.022**	9,529	3,145**	.072	.023**	7,459	3,161*
(CR)^2	−.00083	.00025**	−119	30.6**	−8.6E-4	.00026**	−105	32**
Cum. antitrust	.084	.031**	9.063	2.75**	.099	.034**	7,185	2,681**
S.D. of profit	2.295	.65**	257,523	84,712**	2.637	.83**	−7,445	106,357
Geog. spread.	−.002	.00036**	−179	73*	−.002	.00037**	−12	7.2
Import share	—	—	—	—	−.040	.02	9,134	2,828**
(Imp. share)^2	—	—	—	—	8.6E-4	.00041*	−199	67**
Real wage	—	—	—	—	−.040	.055	33,469	8,223**
BAA bond rate	—	—	—	—	.062	.034	7,052	4,670
IMR	—	—	96,197	44,742*	—	—	91,362	44,136*
Dependent variable	0/1		dollars		0/1		dollars	
Sample	550		357		550		357	
χ^2/R^2	276		.68		284		.78	

[a] Selectivity-bias-corrected OLS with corrected standard errors (Heckman). Data are on 110 three-digit industries for five elections expressed in 1988 dollars; 357 nonzero observations and 193 at 0. In order to generate coefficients with more significant digits, the sales data used in these regressions are entered as one hundred's of millions of 1988 dollars.

*$p \leq .05$.

**$p \leq .01$.

the explanatory variables on two different decisions by industries: the decision to form a PAC and then the decision on the amount that the PAC will spend. The PROBIT equation estimates the probability that the industry has at least one active PAC, and the selectivity-corrected OLS contribution equation predicts the total amount contributed. This approach allows the separate impacts of the independent variables to be identified at each stage. For example, concentration is strongly related to the decision to create a PAC but turns out to have no influence on the amount contributed. The significant concentration coefficients in the PROBIT equation show that higher industrial concentration raises the marginal probability of positive political activity in any industry, up to a concentration ratio of 35%. In contrast, regulation and antitrust indictments are insignificant in the PROBIT equation but positive and significant in the selectivity-corrected contribution model. Regulated industries contribute almost $170,000 more than an (otherwise similar) unregulated industry, and each antitrust indictment raises industry contributions by $9,300, other factors held constant.

Consider now the rest of the results in equation 2. The significant trend coefficient implies that average industry contributions are increasing about $41,000 per year, after controlling for the effect of our independent variables. Private and government sales both increase contributions at a decreasing rate (i.e., coefficients on squared terms are negative), but the effect of government sales is dramatically larger compared with the results from the earlier method. Fifty million dollars in private sales produces about $10,600 in contributions, while an equal amount of sales to the government is associated with $65,500 of industry PAC money.[13] The industries with the most extreme heterogeneity of product lines contribute almost $400,000 less than average. Geographically concentrated industries also contribute significantly less, suggesting that they can use alternate avenues of exercising political influence. Each one-standard-deviation increase in geographic concentration lowers contributions by over $85,000. Finally, industries with greater variability in past profit streams contribute significantly more to political campaigns. Every one-standard-deviation increase in this risk-proxy raises contributions about $40,000.

These results show that the simple organizationally constrained investment model of political activity has considerable explanatory power. We shall add the import, wage, and interest-rate variables to the model and consider the results in more detail. These variables are available only for the 110-industry, 550-observation subsample.

Table 3 adds import share, import share squared, real wages, and the BAA bond rate to the regressors from Table 2, with the sample reduced by 14 industries because of missing data. Equations 1 and 2 reestimate the two-equation selectivity model using the original variable set from the previous analysis.

The only differences from the Table 2 results are that concentration now significantly affects both contribution levels and the probability of participation, and the coefficient on the square of government sales is now positive and significant. The other variables have the same signs as before and are statistically significant.

Equations 3 and 4 in Table 3 contain our results with the four new variables added. Again, compare the standard errors on variables between the PROBIT equation estimating the probability of forming a PAC and the selectivity bias corrected OLS equation estimating the amount of contributions. Regulation and wages are insignificant in the participation decision yet positive and significant for explaining variations in the level of political contributions across industries. On the other hand, geographic concentration and the variability of industry profits are highly significant in the participation equation but much less important in the contribution regression. The BAA bond rate is significant at the .10 level in the PROBIT model, but not in the contribution model. All other variables are significant, at least at the .05 level, in both equations.

We can use the estimates in equation 4 to describe the effect that changes in the variables have on industry political contributions in this subsample. The average level of private sales in this subsample is 834 (meaning the average firm in the average industry has sales of $834 million), which implies contributions of $142,000. An increase of one standard deviation raises contributions to $397,400, an increase of more than a quarter of a million dollars. The contribution-maximizing level of sales is $9.9 billion (only one sample industry has larger average firm sales), implying $888,000 in contributions.

The average level of government sales is $19 million, which is associated with $29,300 in marginal political contributions. A one-standard-deviation increase in government sales raises predicted contributions to $153,800, an increase of more than 400%. These results suggest that at the margin, having government as a customer increases industry political activity more than having private-sector customers. The hypothesis that the two types of sales have identical effects on contributions can be rejected at the .01 level. Regulation also raises contributions more than $100,000 per election, and every antitrust indictment increases industry contributions by $7,200. This last variable ranges from 0 to 35 in this subsample. Greater government involvement with firms either greatly increases the return to political activity or dramatically lowers the costs of collective action by having politicians readily available to coordinate industry activities.

The effect of industry concentration on contributions is increasing up to 35%, just below the sample mean of 41%. At 35% concentration, predicted marginal contributions are about $132,000. A one-standard-deviation increase above 35% (to 53%) reduces the predicted effect of concentration on contributions to only $99,500. At levels of concentration above 70%, the marginal effect of concentration on contributions is negative.[14]

Import share also significantly affects industry PAC contributions. At the sample mean penetration of 10.3%, contributions due to imports are predicted to be about $73,000. A one-standard-deviation increase (to 21.6%) in import penetration raises contributions to about $104,000. Beyond the 23% import level, the marginal effect of import competition on industry contributions begins to fall, reaching zero at around 46%.

Industries that pay higher wages also make greater political contributions. Every one-dollar increase in industry wages increases PAC contributions by around $33,000. Every percentage point increase in interest rates raises each industry's contribution about $7,000, though this effect is not strongly significant. In general, however, our prediction that higher input prices increase political activity is strongly supported in these data.

Geographic concentration is still negative, though only marginally significant in this sample. Each one-standard-deviation increase in geographic concentration lowers contributions about $26,000. The diverse industry dummy is still negative and significant, with a coefficient of −$168,000. These results continue to show the importance of organizational factors on industry political activities.

Finally, the coefficient on the IMR is still positive and significant, showing the importance of our technique's correction for sample selection bias. The general fit of the model is quite good. The R-squared of .79 in the contribution equations means that we are explaining about 80% of the variation in contributions across both industries and time. The PROBIT model predicting the existence of political activity correctly classifies 81% of the 550 cases in the sample. Industry political activity can be explained quite well with a basic model accounting for profit opportunities and free-riding costs.

EMPIRICAL EXAMPLES FROM 1986

We shall consider in more detail the contribution patterns of some selected industries. First, Table 4 contains summary statistics on the independent variables grouped by amount of contributions in 1986 into four categories: no contributions (40 cases), positive contributions less than $100,000 (45 cases), contributions between $100,001 and $500,000 (23 cases), and contributions greater than $500,000 (16 cases). The results show that industries making very similar contributions are often quite heterogeneous. While average sales rises as contributions rise, the standard deviations in each cell for both private and government sales are very large. There is little evidence of a trend across contribution categories for any of the other variables, though both real wages and percentage regulated jump in the highest contribution category. There are however, regulated industries that

TABLE 4

Average Industry Characteristics by Contribution Group, 1986

VARIABLE	NO CONTRIBUTIONS	$1–$100,000	$100,000–$500,000	>$500,000
No. of industries	40	45	23	16
Private sales	207 (220)	460 (453)	1,012 (810)	2,788 (3,033)
Government sales	5.7 (23)	7.0 (11)	15.0 (33)	406 (1,259)
Regulated indust. (% of group)	2.5 (15)	11 (31)	17 (39)	50 (51)
Diverse indust. (% of group)	5.0 (22)	2.2 (15)	4.3 (21)	12.0 (34)
Concentration ratio	37.5 (16)	39.4 (15)	44.1 (16)	48.1 (23)
Antitrust actions	3.05 (7)	3.11 (4)	6.17 (7)	4.56 (6)
Geographic concentration	870 (1,000)	1,100 (1,000)	3,400 (6,000)	1,350 (1,000)
Import share[a]	12.29 (13)	10.14 (12.5)	10.27 (7.5)	5.93 (5.9)
Real wage[a]	10.16 (2.25)	10.48 (1.75)	10.62 (1.46)	12.42 (1.55)

Note: Numbers in parentheses are the standard deviations of the group averages.

[a] Available for the 110-industry sample only.

make few contributions and low-wage industries that make large ones.

Second, we provide five examples of how our model works to predict industry contributions in Table 5. We examine the predicted contributions for the cigarette, aircraft, motor vehicle, trucking, and petroleum-refining industries made by equation 4 in Table 3; that is, the entries give the dollar amount of predicted contributions attributed to each individual independent variable. That number is expressed in thousands of 1988 dollars and is computed by multiplying the value of each variable for each industry by the relevant regression coefficient. We have rounded off the figures to the nearest thousand dollars.

In the cigarette industry (Table 5, col. 1) there are a few, large, geographically dense firms. Actual contributions from this industry in 1986 are $603,000; our regression predicts contributions of $642,000. Private

TABLE 5

Sources of Predicted Contributions (thousands of dollars) in Five Selected Industries, 1986

VARIABLE	CIGARETTES	AIRCRAFT	MOTOR VEHICLES	TRUCKING	PETROLEUM REFINING
Private sales	877	279	484	85	870
Government sales	.0	2,165	171	23	867
Regulated	.0	.0	.0	104	.0
Concentration	−126	52	86	131	130
Antitrust	28	28	163	129	86
Import share	.0	49	115	.0	64
Real wage	461	455	438	418	506
Geographic conc.	−90	−17	−93	−21	−36
Fixed factors[a]	−508	−508	−508	−508	−508
Total predicted contributions	642	2,523	856	374[b]	2,083
Actual contributions	603	2,486	1,092	402	2,715

Note: The technique of using specific industries as examples is drawn from Bartels (1991). The predictions here come from the coefficients estimated earlier in Model 4, Table 3.

[a] Sum of those effects that do not vary cross-sectionally and are therefore fixed for all industries. These include intercept, trend, and interest rate variable.

[b] In this case, the value of the IMR is large enough to raise the predicted contribution significantly ($13,000) above what the reported variables predict, an indication of the importance of what would otherwise be selectivity bias in the estimation procedure.

sales account for $877,000, concentration for −$126,000, and geographic concentration for −$90,000. The aircraft industry, contributed $2,486,000 in 1986, while our model's predicted contribution is 2,523,000. Here, sales to the government is the driving factor, accounting for over 2 million dollars in campaign contributions. Note that neither of these industries is significantly regulated by Pittman's 1977 standards.

The third industry in Table 5 is motor vehicles, or SIC category 371 (vehicle bodies assembly, not final sales). Actual 1986 contributions by this industry are $1,092,000; our predicted contribution is $856,000. In this industry, antitrust trouble and import penetration account for about $280,000 of the total contribution. Sales concentration and geographic concentration offset each other, and government sales explains most of the rest of the prediction. Trucking and courier services is examined in Table 5, column 4. Regulation, sales concentration and antitrust trouble are the main factors driving our predicted contribution of $374,000. The actual industry total is $402,000. Trucking is geographically dispersed, has low average sales to the government, faces no import competition, and has very small average firm size compared to the other industries in our example. Yet trucking contributions approach those of the cigarette industry.

Finally, consider the petroleum refining industry. Petroleum refiners contributed $2,715,000 in 1986, the largest industry contribution in our data. Our model "only" predicts a contribution of about $2.1 million. A large average firm size ($870,000) and significant sales to the government ($867,000) are the largest factors affecting contributions here. However, sales concentration, antitrust trouble, import competition, and high wages also have a significant predicted effect. Our $700,000 underprediction may be indirect evidence supporting Evans's claim that "oil PACs [are] the most ideological" of all corporate PACs (1988, 1048). While our simple cost–benefit model does predict a very large contribution, petroleum refiners contributed even more.

The tenacious reader will have noticed that industry wages are associated with large contribution numbers but have received little emphasis in our exposition of Table 5. That is because there is not much variation in wages across the five chosen industries. Consider a case of two industries similar in many respects but with different average wages. The sawmill industry (SIC 242) contributed $262,000 in 1986, the papermill industry, (SIC 262) $583,000. Both are made up small firms with few sales to the federal government, and neither has much history with the antitrust authorities. Yet there exists (and our model predicts) about a $300,000 difference in their political contributions. Of the difference, $104,000 is due to higher wages in the papermill industry, and another $104,000 is attributed to the fact that papermills are significantly regulated (according to Pittman) and sawmills are not. The rest of the difference is attributed to the greater concentration of the papermill industry.

CONCLUSION

We have investigated the industrial organization of corporate PACs over the period 1978–86. Though the allocation of PAC monies among candidates for political office has been extensively studied, we are the first to present even a simple empirical model of corporate political activity and then investigate *which* industries contribute and *how much* they give. Our empirical results support two theoretical propositions: (1) the evidence is consistent with the notion that industries follow investment-oriented goals in political activity, and (2) the ability of industries to achieve these goals is conditioned by both the benefits that political action brings and the costs of achieving cooperation and organizing collective action. Between 60% and 80% of the variation in contributions is explained in the empirical models. Coefficient signs and significance levels are robust, showing general consistency across time, changes in the sample, and inclusion or omission of independent variables.

We feel our evidence is sufficient to reject the null hypothesis that variations in total PAC contributions are based on idiosyncratic internal goals, at least at the corporate PAC level. Wright's (1985) influential work only directly addresses the behavior of five large trade associations. We argue that attaining real political leverage requires concerted contributions by a group of PACs and therefore study corporate PACs at the industry level. However, it is still an open question whether the factors we use to explain corporate PAC contribution levels can explain total trade association PAC activities. We plan in future work to study the relationships between corporate PACs and industry trade associations.

In closing, one further caveat seems in order. Because our focus has been on organization and collective action problems, we have taken the industry as the appropriate unit of observation. The definition of *industry*, however, has been quite narrow—the collection of corporations within a three-digit SIC code. A more appropriate definition of industry might well include unions and trade associations drawn from the same SIC, reflecting a functional, rather than class, definition. A corporation manufacturing automobiles may share more political goals (trade restrictions on imports, blocking more stringent emission standards, etc.) with the United Auto Workers than with corporations that export semiconductors. If this (currently untested) claim is correct, the traditional division of corporations and unions into political adversaries is not a useful one. The industry, more broadly construed to include all economic actors with an interest in the industry's fortunes, may ultimately prove to be the more useful unit of analysis.

APPENDIX A: THE TROUBLE WITH TOBIT

This appendix discusses the TOBIT technique, which is widely used in political science to solve the problem of a censored sample (particularly in the study of campaign finance; see, e.g., Grier and Munger 1993). Though there are alternate approaches (see Greene 1990, chap. 21 for a good discussion), TOBIT is the most common choice. The problems with TOBIT are not widely understood, however. In particular, TOBIT requires (in our case) that the determinants of *both* the participation and contribution decisions have exactly the same effects.

The classic TOBIT model (Tobin 1958) assumes that there is an index variable y^*, linearly related to the independent variables,

$$y_i^* = X_i\beta + \epsilon_i. \tag{A1}$$

The observed dependent variable y_i is generated as follows:

$$y_i = y_i^* \text{ if } X_i\beta + \epsilon_i > 0;\ \text{otherwise } y_i = 0. \tag{A2}$$

Given that the errors are independently and identically distributed normal, the coefficients of this model can be estimated using maximum likelihood.

As we note in the text, TOBIT models force a single set of coefficients to explain both whether the dependent variable will be observed and its value conditional on being observed. Cragg (1971) points out that it is not obvious that the occurrence of limit observations and the regression model for the observed data should be so closely related. Cragg assumes that the probability of limit observations is independent of the regression model. Greene (1993) shows that this restriction in the TOBIT model can be tested by comparing the fit of the TOBIT model with the combined fit of (1) a PROBIT model predicting the probability of a nonlimit observation and (2) a truncated normal regression on the nonlimit data.

Table A-1 presents some preliminary results, and Greene's test of the TOBIT model versus Cragg's variant. The ordinary least squares regression in equation 1 and the TOBIT in equation 2 have the same signs and significance levels. Equations 3 and 4 (Table A-2) decompose the participation decision and the contribution level into distinct parts. A PROBIT equation is used to predict political activity and then a truncated regression on the 404 positive contributions. The results of these separate procedures can now be compared with the TOBIT model, which assumes the two components are determined in a single equation. Summing the log-likelihood functions of equations 3 and 4 and comparing that sum to the TOBIT log-likelihood produces a chi-squared statistic of 444, which strongly rejects the TOBIT assumption. In the truncated contributions regression, all variables are correctly signed and significant at the .01 level. While none of the substantive conclusions in our empirical work would change if we used the TOBIT model throughout, the model is clearly rejected by the data. The PROBIT and selectivity-corrected least squares regressions used in the text provide better information.

TABLE A-1

Testing the Tobit Restrictions

INDEPENDENT VARIABLES	EQUATION			
	1 (OLS)	2 (TOBIT)	3 (PROBIT)	4 (TRUNCATED)
Constant	−68,020 (21,801)**	−151,609 (47,675)**	−.216 (.26)	−1,760,330 (271,656)**
Trend	23,709 (6,425)**	33,685 (8,772)**	.120 (.047)*	106,914 (10,586)**
Private sales	11,568 (639)**	14,069 (832)**	.196 (.026)**	19,452 (1,926)**
Government sales	27,601 (2,763)**	33,470 (3,630)**	.641 (.33)*	61,368 (8,754)**
Regulated industry	156,234 (26,891)**	168,418 (35,160)**	.327 (.26)	698,545 (127,240)**
Diverse industry	−151,056 (50,690)**	330,420 (79,428)**	−1.157 (.38)**	−785,631 (262,752)**
Concentration ratio	1,134 (545)*	1,685 (736)**	−.008 (.0053)**	12,335 (2,452)**
Cumulative Antitrust	5,205 (2,303)*	8,160 (3,022)	.009 (.17)	38,440 (9,636)**
S.D. of profit	149,138 (47,800)**	287,391 (60,631)**	3.132 (.6)**	529,299 (150,797)**
Geographic spread	−169 (40)**	−454 (62)**	−.002 (.0003)**	−857 (271)**
Dep. var.	dollars	dollars	0/1	dollars
Sample size	620	620	620	404
R^2/log-likelihood	.56	−5,728	−265	−5,241

Note: Data are on 124 three-digit industries for five elections expressed in 1988 dollars. There are 404 nonzero observations and 216 at 0. Numbers in parentheses are standard errors. To generate coefficients with more significant digits, the sales data used in these regressions are entered as one hundred's of millions of 1988 dollars.

*$p \le .05$.

**$p \le .01$.

APPENDIX B: DESCRIPTIONS AND SOURCES FOR VARIABLES

The industrial concentration, regulation, and import share variables do not vary over time, and the interest rate variable does not vary cross-sectionally. Wages and profits only vary at the two-digit level; that is, in any year all three-digit industries with a common two-digit home are assigned the same wage and profit numbers.

Total Industry Contributions (Dependent Variable). Each corporation on the COMPUSTAT data base was matched against the Federal Election Commission master list of corporate PACs (Nonparty Spread File, Reports on Financial Activity) for each election year in the sample (1978, 1980, 1982, 1984, 1986). If a corporation was not identified as having a PAC, the contributions for that firm were listed as zero; otherwise, the total contributions of the PAC to House candidates were listed. Many PACs were neither identified as having corporate sponsors, nor had an identifiable company name revealed in their registered PAC title. These PACs were dropped from the sample. The firm-level data were then aggregated up to the three-digit SIC industry level, using the SIC codes listed on COMPUSTAT, by adding up all the contributions within an industry.

Average Private Sales. Obtained from COMPUSTAT; item no. 12 on industrial database. Gross sales (the amount of actual billings to customers for regular sales completed during the period) reduced by cash discounts, trade discounts, and returned sales and allowances for which credit is given to customers. Value is in millions of dollars and is annual (as are all COMPUSTAT variables). Our variable represents the average of sales over firms in the three-digit industry.

Average Government Sales. Data on proportion of industry total sales that went to the federal government were obtained from Russell Pittman; original data came from Bureau of the Census 1985a–d. This proportion was multiplied by industry total sales to yield an estimate of dollar sales to the federal government.

Regulated Industry. Dummy variable, equal 1 if industry is regulated. Obtained from Pittman 1977 for four-digit industries. If the three-digit industry contained at least one regulated four-digit industry, it was considered regulated.

Concentration Ratio. Data were obtained from Russell Pittman; actual values represent corrected (by Department of Justice) concentration ratios for 1982, with original data coming from Bureau of the Census 1985a.

Cumulative Antitrust Indictments. Data were obtained from Gerry Miller and used in Miller 1989 and Miller, Shughart, and Tollison 1990. These data were originally compiled by Miller under a Freedom of Information Act request from the Department of Justice and Federal Trade Commission. Miller tracked indictments of all firms and made the data available to us at the four-digit SIC level. We aggregated up to the three-digit level and created a moving (cumulative) sum of industry violations.

Geographic Concentration. The variable is a Herfindahl index, calculated for each industry *j* as Σi (Sales$_i$/Sales)^2, where sales in the *i*th state are calculated using the COMPUSTAT state IDs for each corporation in the sample, and the denominator of the ratio is total industry *j* sales.

Industry Profits. The variable is "corporate profits without adjustments" contained in the Bureau of Economic Analysis Income and Wealth Division's gross product originating (GPO) by industry, table 6.1F. These data are annual, available in an industry grouping very similar to the two-digit SIC. We adjusted these data for inflation using industry-specific price indices and gave the appropriate two-digit profit figure to each three-digit industry in the sample.

Diverse Industry. Source data come from COMPUSTAT Industrial Segment File, where the number of segments (roughly, products) are reported by each firm, up to a maximum of 10. We averaged these data by industry (mean = 1.7, standard deviation = .65) and then defined a "diverse industry" as one with mean number of products more than two standard deviations above the sample average; that is, all industries whose firms average more than three product lines are coded 1.0 and the others .00.

Import Share. Data were obtained from Don Clark and used in Clark, Kaserman, and Mayo 1990. Import share is defined as total imports divided by total shipments for the industry. Original data were four-digit SIC level, aggregated up to the three-digit level.

Wage Rate. From CITIBASE data bank. Variables are "average hourly earnings of production workers" (LE6M20–LE6M39), corresponding to two-digit SICs. We adjusted for inflation using the gross national product deflator and gave the appropriate two-digit "real wage" figure to each three-digit industry in the sample.

BAA Bond Rate. From CITIBASE data bank. Values are the annual average rate charged on BAA rating bonds, from 1978 through 1986.

TABLE B-1

Data Summary

VARIABLE AND NO. INDUSTRIES	MEAN	S.D.	MINIMUM	MAXIMUM
Contributions				
124	140,010	326,940	.00	2,715,400
110	130,110	313,250	.00	2,715,400
Avg. private sales (millions)				
124	796.37	1,646	2.81	18,519
110	834.32	1,729	2.81	18,519
Avg. government sales (millions)				
124	49.82	363	.00	5,087
110	18.94	69	.00	629
Concentration Ratio (4-firm)				
124	40.81	17.08	10.00	93.00
110	41.08	16.20	10.50	86.00
Cum. antitrust				
124	2.54	4.03	.00	44.00
110	2.56	3.55	.00	35.00
S.D. of Profit				
124	.151	.18	.007	2.24
110	.138	.12	.007	.57
Regulated indust.				
124	.14	.35	.00	1.00
110	.12	.32	.00	1.00
Diverse indust.				
124	.048	.214	.00	1.00
110	.045	.208	.00	1.00
Geographic Herf. index				
124	3,929	2,245	636	9,925
110	3,868	2,222	636	9,925
Wave rate				
110	10.57	1.91	6.23	15.14
Import share				
110	10.28	11.33	.00	56.10
BAA bond rate				
110	12.55	2.13	9.45	15.34

APPENDIX C

This appendix gives information about the industries used in the sample and their total contributions over the period 1978–1986. The industry definitions and descriptions are taken from the Department of Commerce Standard Industrial Classifications, using 3-digit industries. The contribution totals are the sum of contributions reported to the Federal Election Commission, by corporate PAC, aggregated into these industry definitions. The dollar amounts were all transformed to real (1988 dollar) values, and then summed from 1978–86.

109 Miscellaneous Metal Ores (0)
131 Petroleum & Natural Gas Extraction (1,608)
138 Oil & Gas Field Services (978)
162 Heavy Construction, Except Highway (0)
201 Meat Products (531)
202 Dairy Products (157)
203 Preserved Fruit & Vegetables (0)
204 Grain Mill Products (838)
205 Bakery Products (1,225)
206 Sugar & Confectionary Products (70)
207 Fats & Oils (505)
208 Beverages (2,879)
209 Misc. Foods & Kindred Products (25)
211 Cigarettes (1,358)
221 Broadwoven Textile Mills (cotton) (483)
222 Broadwoven Textile Mills (manmade) (70)
225 Knitting Mills (26)
227 Carpets & Rugs (42)
232 Men's & Boy's Furnishings (142)
233 Women's & Misses' Outerwear (0)
234 Women's & Children's Undergarments (0)
239 Misc. Fabricated Textile Products (3)
242 Sawmills & Planing Mills (863)
243 Millwork, Plywood & Structural (906)
245 Wood Buildings & Mobile Homes (0)
251 Household Furniture (0)
252 Office Furniture (7)
253 Public Building & Related Furniture (0)
259 Misc. Furniture & Fixtures (0)
261 Pulp Mills (0)
262 Paper Mills (2,817)
263 Paperboard Mills (76)
265 Paperboard Containers & Boxes (10)
271 Newspapers (0)
272 Periodicals (109)
273 Books (0)
275 Commercial Printing (214)
276 Manifold Business Forms (9)
277 Greeting Cards (0)
278 Blankbooks & Bookbinding (11)
279 Printing Trade Services (0)
281 Industrial Inorganic Chemicals (586)
282 Plastics Materials & Synthetics (1,144)
283 Drugs (4,580)
284 Soap, Cleaners & Toilet Goods (328)
285 Paints & Allied Products (355)
286 Industrial Organic Chemicals (35)
287 Agricultural Chemicals (516)
289 Misc. Chemical Products (184)
291 Petroleum Refining (10,525)
295 Asphalt Paving & Roofing Materials (0)
301 Tires & Inner Tubes (167)
302 Rubber & Plastic Footwear (117)
306 Fabricated Rubber Products (46)
314 Nonrubber Footwear (1)
321 Flat Glass (0)
322 Glass & Glassware, Pressed/Blown (640)
323 Products of Purchased Glass (0)
324 Cement, Hydraulic (228)
325 Structural Clay Products (109)
326 Pottery & Related Products (0)
327 Concrete, Gypsum & Plaster Products (164)
329 Misc. Nonmetallic Mineral Products (109)
331 Blast Furnace & Basic Steel Products (1,654)
332 Iron & Steel Foundries (92)
333 Primary Nonferrous Metals (1,349)
334 Nonferrous Products
335 Nonferrous Rolling & Drawing (149)
336 Nonferrous Foundries (8)
339 Misc. Primary Metal Products (0)
341 Metal Cans & Shipping Containers (215)
342 Cutlery, Hand Tools & Hardware (18)
343 Plumbing & Heating (Nonelectric) (21)
345 Screw Machine Products (0)
346 Metal Forgings & Stampings (33)
347 Metal Services (51)
348 Ordnance & Accessories (0)
349 Misc. Fabricated Metal Products (0)
351 Engines & Turbines (134)
352 Farm & Garden Machinery (2,627)
353 Construction & Related Equipment (915)
354 Metalworking Machinery (48)
355 Special Industry Machinery (22)
356 General Industrial Machinery (736)
357 Computer & Office Equipment (711)
358 Refrigeration & Service Machinery (148)
359 Industrial Machinery (137)
361 Electric Distribution Equipment (990)
362 Electrical Industrial Apparatus (261)
363 Household Appliances (434)
364 Electric Wiring & Lighting Equipment (0)
365 Household Audio & Video Equipment (267)
366 Communications Equipment (996)
367 Electronic Components & Accessories (450)
369 Misc. Electrical Equipment & Supplies (145)
371 Motor Vehicles & Equipment (4,013)
372 Aircraft & Parts (8,505)
373 Ship & Boat Building & Repair (0)
374 Railroad Equipment (16)
375 Motorcycles, Bicycles & Parts (25)
376 Guided Missiles, Space Vehicles & Parts (4,233)
379 Misc. Transportation Equipment (0)
381 Search & Navigation Equipment (1,934)
382 Measuring & Controlling Devices (472)
384 Medical Instruments & Supplies (47)
385 Ophthalmic Goods (0)
386 Photographic Equipment & Supplies (200)
387 Watches, Clocks, Watchcases & Parts (0)
391 Jewelry, Silverware & Plated Ware (40)
394 Toys & Sporting Goods (153)
395 Pens, Pencils, Office & Art Supplies (0)
396 Costume Jewelry & Notions (0)
399 Misc. Manufactures (0)
401 Railroads (2,636)
421 Trucking & Courier Services (Nonair) (1,026)
422 Public Warehousing & Storage (0)
441 Deep Sea Foreign Freight Transportation (171)
451 Air Transportation, Scheduled (2,992)
461 Pipelines, Except Natural Gas (69)
481 Telephone Communications (5,116)
483 Radio & Television Broadcasting (48)
491 Electric Services (2,580)
492 Gas Production & Distribution (2,492)
493 Combination Utility Services (1,355)

Notes

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1. An exception is Hall and Wayman 1990. They find that the use of time by committee members is strongly correlated with contributions.

2. Andres (1985) examines the Fortune 500 firms for 1980 (though his sample inexplicably contains only 426 observations). Masters and Keim (1985) and Humphries (1991) examine the Fortune 1000 manufacturing firms in the year 1982. Masters and Keim add about 150 nonmanufacturing firms, as well. Grier, Munger, and Roberts (1991) examine the percentage of firms with PACs in 96 manufacturing industries (regardless of size, provided the firms are listed in the Compustat database) in 1984. McKeown (1994) examines 118 firms at three different points in time.

3. There are a variety of measures of industry structure in

the economics–industrial organization literature, with the most common being the four-firm concentration ratio. This ratio can be defined as the sum of total sales of the largest four firms in the industry, divided by the total sales of the entire industry. Obviously, a concentration ratio of one means that there are only four (or fewer) firms. A concentration ratio of .10 would mean that the largest four firms account for only 10% of the total sales (and hence market power) of the industry.

4. Boies chooses 10 variables by looking at pairwise coefficients, then derives a four-variable model from stepwise OLS regressions, in effect using two pretest estimators, each of which are biased and inconsistent.

5. It is worth pointing out that the empirical model we will estimate based on this abstract profit function is a reduced form, capturing the aggregate effects of variables that may effect both the costs and the benefits of political action. The main reason we specify a formal profit function at all is to justify inclusion of the input price variables, which are then implied as direct comparative statics results of the model.

6. We subscript $I(\phi)_i$ because different industries will exercise very different levels of influence depending on the political support among voters—and other industries—for the policy or service in question. This function is designed to focus just on the marginal impact of PAC contributions by the industry.

7. The exact mechanism by which influence is obtained through contributions is left unspecified here. Two possible modeling approaches are Denzau and Munger 1986 and Baron 1994.

8. The evidence on corporate PACs (Eismeier and Pollock 1988; Handler and Mulkern 1982, Matasar 1986, Morrison 1992), based on both perceptions of researchers and interviews with company executives and lobbyists, would appear to indicate that corporate PACs are fairly directly controlled by the sponsoring organization. This top-down control is quite different from the looser and more anarchic setting in which trade associations work (Bauer, Pool, and Dexter 1963; Wright 1985). Further, as Grier and Munger (1991) show, the allocations of corporate and trade association PAC contributions are statistically distinct and must be analyzed separately.

9. The general form of the Herfindahl index is the sum of the squares of some fractions that sum to one. It is widely used to measure market structure (the sum of the squares of the market shares of all firms). In this case, the "share" variable is the proportion of total sales that each state represents for the industry.

10. As Snyder (1989) shows, the issue of optimal dispersion of "power" is a difficult one. The issue depends in part on the ability of a group (e.g., in this case, of employees at manufacturing plants) to control a single district or to spread among several districts. The probability of being pivotal in multiple districts is higher if the workers are spread out; the probability of being pivotal in at least one district is obviously higher if all the workers are in one district. It may not be true, therefore, that geographic diversity has a monotonic effect in either direction.

11. The average number of product lines per industry is 1.7. The cut-off point for being considered a "diverse" industry is 3.0. We ran regressions using product lines directly. In a linear specification, it is negative but insignificant; adding the square turns the linear coefficient positive; and the square coefficient is negative and significant. Since it seems strange to estimate a quadratic function for a variable that ranges from 1 to 4.1, we used the dummy variable described in the text.

12. Standard OLS estimates are inconsistent, and the standard errors are incorrect (see Greene 1993; Heckman 1979). The standard errors reported here are corrected, using the procedure Greene derives from Heckman's consistent estimator. All our regressions were estimated with LIMDEP version 5.1 and checked with SHAZAM version 7.0.

13. The average level of sales to the federal government in the full sample is $50 million.

14. These results are broadly similar to those of Grier, Munger, and Roberts (1991), who find that the influence of concentration on the number of PACs in an industry is nonlinear, first increasing and then decreasing, for the year 1984.

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REPORT FROM THE LABORATORY: THE INFLUENCE OF INSTITUTIONS ON POLITICAL ELITES' DEMOCRATIC VALUES IN GERMANY

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The unification of Germany revives several questions about the future of Germany's democracy. Given the socialist–authoritarian background, how supportive are East Germany's elites of liberal democratic rights? Has the socialist–democratic experience instilled into elites a social egalitarian conception of democracies? In what ways, if at all, do elites support direct democracy procedures? I examine political elites' conceptions of democracies in the united Germany in 1991, using a survey of 168 parliamentarians from the united parliament in Berlin. I find that the socialist and parliamentary institutions in the East and the West, respectively, have substantially influenced elites' conceptions of democracies in Germany, leading to a value divergence across the East–West boundary. Yet the findings also suggest that a partial value convergence in terms of liberal democratic rights among postwar elites has taken place. The results support an institutional learning theory, but they also suggest that support for liberal democratic values has been diffused into East Germany.

> I think entirely in dialectical terms.
>
> —an East Berlin CDU parliamentarian
> on evaluating political issues

The unification of Germany revives several questions about the future of Germany's democracy, most of which analysts of the West German political system had thought were resolved. Especially throughout the 1980s, a consensus evolved among scholars that the West German mass public and elites had developed a democratic political culture (Baker, Dalton, and Hildebrandt 1981; Hoffmann-Lange 1985; Roth 1976; Weidenfeld and Korte 1991). However, the socialist East German system in many ways represented a continuation of Germany's antidemocratic past—one-party-controlled, with few opportunities for elites and the public alike to express their opinions freely (Childs, Baylis, and Rueschemeyer 1989; Krisch 1985; Ludz 1972; Schweigler 1975). Hence, given the importance of political elites in the political reconstruction of the unified Germany, I shall analyze the democratic values of the German political elite at the end of 1991.

There is no agreement on one fixed blend of democratic values necessary to make a political elite democratic. But the extent of elites' commitment to basic democratic rights, their views on social egalitarian principles, and the extent to which plebiscitarian procedures are favored by elites partly define the character that a democratic system develops (Dahl 1989; Pennock 1979). These three core values (also referred to as *elites' conceptions of democracies*)[1] constitute important elements in elites' ideology (Collier and Mahon 1993; Putnam 1973). Thus, given the 50-year history of experience in an authoritarian regime, how supportive are East German elites of liberal democratic rights (e.g., free elections, freedom of discussion)? Has the socialist experience instilled into East German elites a social egalitarian conception of democracy? Given the extensive involvement of ordinary citizens in the 1989 revolution and the emphasis of socialist ideology on direct democracy, how do East German elites view plebiscitarian procedures? Using a survey of East and West German parliamentarians from the united parliament in Berlin, I shall attempt to address these questions.

Although my focus is on legislative elites' conceptions of democracies in Berlin shortly after unification, the theoretical significance reaches beyond Berlin and, indeed, beyond Germany; for it addresses the perennial question of whether political institutions can reshape elites' core values. Previous elite studies remain inconclusive on this issue because they focus *either* on individual-level *or* systemic source of elites' political values. For example, individual-level studies highlight elites' professional norms as sources of parliamentary behavior (Herzog et al. 1990; Searing 1982, 1985) and elite attitudes (Aberbach, Putnam, and Rockman 1981; Roth 1976); or else they document the influence of elites' generational experience on elite polarization over such New Politics issues as environmentalism (Dalton 1987).[2] Systemic studies, in contrast, assume that the unique regime experience of elites within a given polity (e.g., political institutions, level of economic development) contributes to a similar outlook on core political values (Di Palma 1990; Higley and Burton 1989; Higley and Gunther 1992).[3]

Despite the recognition that both individual-level and systemic factors are important sources of elite values, these two perspectives have not been combined into a single study. Individual-level studies generally do not incorporate national-level factors into their empirical analyses of elites' values and frequently treat core values as an "uncaused first cause" (Putnam 1973, 149). In contrast, systemic approaches cannot generate the detailed empirical data of survey studies about elites' values because such information for longer time periods (frequently centuries) is not available.[4] The main reason for this gap is undoubtedly the absence of proper cases where both types of factors can systematically be varied, measured, and controlled for. I suggest that the conditions created by Germany's division and unification offer the opportunity for a synthesis by examining the influence of political institutions (controlling for individual-level East–West differences) on elites' conceptions of democracies.

Thus one of my theoretical objectives is to analyze the diverging impact of socialist and parliamentary institutions on elites' democratic values. But a second is to consider the possibility that democratic ideals have spread to East Germany. In fact, there are good reasons to argue that East and West Germans have not merely drifted apart during the past four decades. Analysts of democratic transitions in Southern and East–Central Europe have argued that support for liberal democratic values may develop even within authoritarian states (Di Palma 1990; McDonough, Barnes, and Lopez-Pina 1986; Weil 1989). While these studies point to a broad range of potential sources of value diffusion (e.g., mass media, economic development, education), the diffusion argument predicts that democratic values have developed in East Germany despite the authoritarian political environment. I will therefore also examine the possibility of a value convergence in terms of elites' conceptions of democracies.

In seeking to address these issues, I interviewed 168 parliamentarians from the united Berlin parliament (79 from East Berlin, 89 from West Berlin) between September 1991 and June 1992. I view Berlin's division and unification as a quasi laboratory, where the effect of socialist and parliamentary-democratic systems on elites' conceptions of democracies can be studied. Although a focus on the Berlin parliament lacks the external validity of a national elite study, it has the advantage of maximizing the range of background factors that are being kept constant (e.g., regional differences in political traditions within Germany between South–West and North–East). Further, where available, I will present evidence that the Berlin results are generalizable to the national legislative elite. I therefore believe that the Berlin study provides a first important step toward assessing systematically the influence of political institutions on elites' understanding of a democracy.

First, I argue that a process of institutional learning may have substantially shaped elites' democratic ideals in East and West Germany. Based on this discussion, I shall propose a series of testable hypothesis. Next, I shall present evidence that corroborates several (though not all) institutional-learning hypotheses. Finally, I shall examine the extent to which democratic values have been diffused into East Germany and highlight the main implications of this study.

INSTITUTIONAL LEARNING AND DEMOCRACY IN GERMANY

The establishment of a parliamentary democracy in West Germany and a socialist system in East Germany in 1949 marks the beginning of a monumental social experiment; for in both systems, the political culture had to be reconstructed to conform to the new regimes. In West Germany, public opinion and elite studies documented that the undemocratic forces during the 1950s had to be reckoned with (Almond and Verba 1963; Edinger 1960; Meritt and Meritt 1970). But the longevity of democratic institutions and the economic success during the 1950s and 1960s also began to transform, slowly but steadily, mass and elite political values. By the mid-1970s, analysts of public opinion and political elites alike began to document the growing acceptance of democratic values, especially among citizens born during the postwar years (Baker, Dalton, and Hildebrandt 1981; Boynton and Loewenberg 1973, 1974; Hoffmann-Lange 1985; Kaase 1971; Kuechler 1992). In East Germany, while Communist party elites could partly benefit from the authoritarian traditions in German society (Dahrendorf 1967), the task was to imbue East Germans with socialist ideals (Glaessner 1989). Although the developments in East Germany are less well documented than those in West Germany, initial analyses seem to indicate that the East German system was at least partially successful in achieving this goal (Bauer 1991; Friedrich 1990; Noelle-Neumann 1991).[5]

Theoretically, the division-and-unification of Germany allows analysts to examine whether a political system, if established for a sufficient time period, can reshape a nation's political culture. Almond and Verba's (1963) seminal *Civic Culture* analyzes the culture–institution nexus by primarily examining the value prerequisites of democratic systems. But Verba, in an important essay, explicitly suggests that institutions may also have a significant influence on individuals' values because "what an individual believes about the political process is learned from observation of that process" (1965, 533). The central supposition is that elites' (and publics') exposure to a particular institutional configuration substantially influences the basic political values of individuals, a theme that became increasingly important in the wake of the success of West German democratic institutions (Almond and Verba 1980; Conradt 1974; Rogowski 1974). From this perspective, individuals' regime experience may be viewed as a combination of elites' exposure to the regime ideology (the official values and norms) and regime practices (the operating norms). At the center of this perspective, then, is the premise that the process of institutional learning molds individuals' democratic values.[6]

The effect of newly established institutions on elites' political values is an especially important issue in contexts where abrupt and fundamental changes in political systems occur, such as the ones in Central

Europe after World War II or in 1989. Are new institutional arrangements able to imbue citizens with those ideological values that institutions need to endure? Political culture theory predicts that under these conditions individuals' existing value predispositions will initially delay the impact of new institutional learning (Almond and Verba 1963).[7] Yet, one would also expect that individuals will slowly but steadily incorporate the new institutional values and norms into individuals' preexisting values. In particular, generational experience may become a "major basis for subcultural differentiation" (Eckstein 1988, 798). That is, cohorts born after a regime transition are only exposed to the new institutional configuration and should provide less resistance to the new institutional values and norms than generations that were also shaped by pretransition institutions (Sigel 1989). This perspective therefore anticipates that elites' core values in West *and* East Germany should be influenced by the democratic and socialist regime experience because at least two elite generations have been socialized under two fundamentally different systems since about 1949. In short, the institutional learning perspective, combined with the notion of generational change, predicts that the East German socialist system, *despite* its collapse, shaped elites' ideological predispositions.[8]

Democratic Ideals in East and West Germany

Based on these considerations, the division of Germany after World War II and the 1989 unification can be used to study the effect of parliamentary-democratic and socialist institutions on elites' political values under quasi experimental conditions. Since a diverse range of factors (e.g., social culture, historical traditions) that usually varies across nations remains constant, these systemic factors can be largely ruled out as sources of East–West variations in elites' democratic values. Further, all parliamentarians represent constituencies within the city of Berlin, which means that variations of regional interests (e.g., between cities and more rural areas) are unlikely to enter parliamentarians' views on the topics discussed with them (a "most–most" similar systems design).[9]

Undoubtedly, there are other national-level variations between East and West Germany that our study design cannot adjust. For example, the West German economy has clearly outperformed East Germany's command economy. However, although this East–West difference may influence elites' views on specific economic decisions (e.g., where to invest resources for a particular project), it is less plausible to view differences in economic capacity as sources of ideological differences across the East–West boundary. Recognizing that the design only approximates—but does not duplicate—experimental conditions, I submit that variations of elites' ideological values, if they exist, must be attributed to the different institutional configuration that elites from East and West Berlin were exposed to.

Following the work of political theorists (e.g., Dahl 1989; Held 1987; Pennock 1979), I examine elites' democratic values from three different angles and propose six hypotheses about the expected East–West and generational differences. Briefly, liberal parliamentary democracies emphasize individual political rights in their conceptions of democracies, such as the right to participate in politics and the right to form an opposition (what Dahl terms *polyarchy*). Second, socialist democracies emphasize the egalitarian distribution of economic goods and link the ideal-typical democracy to the egalitarian provision of economic goods (Held 1987). Finally, a direct democracy model stresses the development of plebiscitarian procedures to expand the direct involvement of citizens (Barber 1984; Dahl 1989). These three different conceptions are not mutually exclusive, but the particular emphasis on these values defines elites' ideal-typical democracy.

Based on these variants and the institutional learning argument, the first expectation concerns elites' support for liberal democratic rights. While the ideological and institutional foundations of the German Democratic Republic (GDR) differed from previous authoritarian regimes, several analysts maintain that the authoritarian structures of the socialist East German state resemble past authoritarian institutions (Dahrendorf 1967; McCauley 1983). For example, East German elites were socialized in a political environment that penalized the open and regime-critical exchange of political ideas. In addition, the official rhetoric of the GDR regime did not nurture political competition or independent mass involvement in politics. In contrast, West German elites have been socialized into a political system that encourages political competition and citizens' participation in politics. Thus, the first proposition focuses on elites' support for liberal democratic rights:

> HYPOTHESIS 1. *East German elites are less supportive of liberal democratic rights than West German elites.*

Furthermore, postwar elites in West Germany were socialized under democratic conditions, whereas prewar elites also experienced the authoritarian influence of the Third Reich. In contrast, prewar and postwar elites in East Germany were primarily exposed to authoritarian systems. Therefore, the effect of institutional learning should reflect generational differences within West Germany but not in East Germany (Eckstein 1988):

> HYPOTHESIS 2. *Postwar elites are more committed to democratic rights than prewar elites in West Germany, but not in East Germany.*

The socialist system in East Germany permanently exposed East German citizens to an ideology that closely linked a democracy to the egalitarian distribution of economic goods. This ideological message was reinforced in schools or through the minimal opportunity to own private property (Glaessner 1989; Rueschemeyer and Lemke 1989):

HYPOTHESIS 3. *East, but not West, elites' conceptions of democracies stress social egalitarian elements.*

The logic of generational experience implies a further proposition about the anticipated cohort differences regarding the understanding of democracy by members of parliament (MPs):

HYPOTHESIS 4. *Postwar elites in East Germany, but not in West Germany, are more likely than prewar elites to emphasize the social egalitarian conception of democracies.*

Hypotheses 5 and 6 concern East elites' views on direct democracy procedures. There are two reasons to believe that East Germans are more likely than West Germans to support plebiscitarian procedures. First, the extensive involvement of citizens during the 1989 revolution undoubtedly sensitized East elites to the importance of citizens' involvement in politics. Had it not been for the so-called Monday demonstrations of the East German public, the upheavals in East Germany during 1989 would likely have taken a different course. Second, while socialist systems did not endorse independent involvement of masses in politics, socialist ideology did endorse the direct mobilization of masses for socialist goals (Held 1987). While I will not be able to disentangle the relative importance of these processes, the logic of these arguments provides the basis for the fifth hypothesis:

HYPOTHESIS 5. *East German elites are more supportive of plebiscitarian components than West Germans.*

Finally, while all citizens in East Germany experienced the collapse of their political regime, postwar elites should be particularly affected by this experience because younger citizens were disproportionately involved in bringing down the socialist system. In addition, the postwar cohort in East Germany was exclusively exposed to the mobilization tactics of the socialist regime. In West Germany, the New Politics literature suggests that postwar elites in West Germany should be more supportive than prewar elites of plebiscitarian procedures (Dalton 1992; Inglehart 1990):

HYPOTHESIS 6. *Postwar elites in East and West Germany are more likely than prewar elites to endorse procedures that increase the direct involvement of citizens in politics.*

These six hypotheses make the institutional-learning argument testable, but they also define the benchmark expectations to uncover value domains shaped by converging forces. That is, if I do not find differences where, on the basis of institutional learning, I expect them, I must explain their absence.

THE DATA

The data stem from a survey conducted between September 1991 and June 1992 with 168 (about 70%) parliamentarians of the united Berlin parliament. West MPs have been socialized under the West German system,[10] and they have spent an average of about seven years in the Berlin parliament. In addition, there is no reason to believe that West MPs are unrepresentative in terms of their democratic conceptions of the general political elite in West Berlin. However, I suspect that the nature of the selection process of East Berlin MPs may have biased the composition of East Berlin MPs. Most importantly, one criterion in selecting candidates for the Berlin parliament was the absence of close ties to the old GDR regime. Individuals who were politically active in the GDR system were unlikely to be nominated for a legislative seat within the new political system. Even within the reformed communist party (PDS), it furthered a candidate's career after the transition not to hold visible political or social positions in the GDR. Undoubtedly, several lower-level functionaries of the GDR have joined the West German parties. Still, the contemporary elite in East Germany, on average, probably overrepresents the most democratic forces within the East. Although this bias suggests caution in generalizing my findings to East Germany's old political elite, it actually stacks the case against confirming the hypotheses because old GDR elites were most intensely exposed to the socialist learning process. Thus any East–West difference that this study produces might actually be larger if old GDR elites were represented in adequate proportions in the Berlin parliament.

The interviews are based on a structured questionnaire containing a combination of open-ended and closed-ended questions. The Appendix contains detailed information on the data collection process and discusses evidence that the interviewed MPs closely mirror key characteristics of the entire Berlin parliament.

RESULTS

I employed several measures in order to investigate elites' support for liberal democratic, egalitarian, and plebiscitarian principles. To measure MPs' support for liberal democratic rights, I asked MPs to indicate their agreement or disagreement with items reflecting basic principles of liberal democracies (Table 1). I first present the bivariate results, followed by multivariate analyses, because the bivariate results convey a detailed profile of East and West elites' democratic values which the multivariate analyses does not convey. The first three items (A, B, C) measure individuals' commitment to general democratic procedures, while the next three indicators present respondents with a trade-off between conflict and order (D, E, F) (Kaase 1971; Putnam 1973). There is virtually unanimous support for the most basic democratic rights (A, B, C). Yet the level of support for democratic rights is significantly reduced when they conflict with another goal—for example, moral or ethical considerations (D) or the stability of the political order (E). Furthermore, there is some evidence that elites in the East are somewhat more willing to curtail individuals'

TABLE 1

MPs' Support for Democratic Rights

MEASURES		EAST	WEST	GAMMA
A.	Every democracy requires a political opposition.	100.0	100.0	(a)
B.	It is the primary duty of the political opposition to support the government, and not to criticize it.	8.9	13.5	.16
C.	Every citizen has the right to demonstrate.	100.0	97.8	(a)
D.	Freedom of opinion and discussion must be limited by moral and ethical consideration.	57.0	42.7	−.27
E.	A citizen forfeits the right to demonstrate and to strike when he or she threatens the political order.	31.6	30.3	−.09
F.	The freedom of political propaganda is not an absolute freedom, and the state should carefully regulate its use.	29.1	23.6	−.22

Note: Entries are percentages of respondents who agree with statements. See the Appendix for question wording and recodes. N = 79 for East Berlin, 87 for West Berlin.
(a) Too little variation to compute gamma.

rights when order-related objectives conflict with liberal democratic rights. On the whole, while East German MPs are somewhat less supportive of these rights than West German MPs, both elite sectors evidence similar levels of support for liberal democratic rights.

Despite the agreement among East and West elites over basic political rights, it would be premature to conclude that East and West German elites are essentially alike. One shortcoming of closed-ended items is that they may mask considerable differences in elites' conception of an ideal-typical democracy. Accordingly, I asked respondents to define the core elements in their own words: "The term democracy is frequently used without further specifications these days. What seem to you personally the essentials of a democracy?"

This open-ended question elicited a wide range of responses, but the basic patterns can be summarized as indicated in Table 2. (The Appendix contains the original code categories and recodes used for this table). Since the later multivariate analyses use this question only to measure egalitarian ideals, this table also displays cohort differences within the East and West.

A large number of MPs mention at least one civil right (e.g., liberty, freedom of expression) as an important component of a democratic system, both in the East (64.6%) and the West (73.0%). Within the East, prewar generations are more likely to mention civil rights (77.1%) than postwar generations (55.9%), whereas there are no clear generational differences among MPs from West Berlin. Similarly, a quarter of East and West MPs mention at least one "government-by-the-people" item (e.g., popular control). Consistent with the results from the analysis of closed-ended indicators, both East and West MPs evidently value basic civil rights and popular control as important ingredients of a democracy.

Yet differences between East and West MPs emerge in regard to the social equality component. East MPs are substantially more likely than West MPs to mention the necessity for governments to provide social security to individuals. Over a third of East Berlin MPs argue that guaranteeing a just standard of living

TABLE 2

MPs' Conceptions of Democracy, by Generation

DEMOCRATIC COMPONENT	EAST	WEST
Government by the people	26.6	25.8
Prewar	24.4	26.1
Postwar	29.4	25.6
Active participation	17.7	12.4
Prewar	13.3	13.0
Postwar	23.5	11.0
Direct democracy	25.3	2.2
Prewar	13.3	.0
Postwar	41.2	4.7
Social equality	35.4	7.9
Prewar	28.9	10.9
Postwar	44.1	4.7
Equality of opportunity	3.8	7.9
Prewar	2.2	4.3
Postwar	5.9	11.6
Civil rights/limited government	64.6	73.0
Prewar	77.1	69.6
Postwar	55.9	76.7
Institutions	40.5	60.7
Prewar	51.1	58.7
Postwar	26.5	62.8
Political competition	20.3	30.3
Prewar	22.2	32.6
Postwar	17.6	27.9
Societal competition	17.7	19.1
Prewar	17.8	15.2
Postwar	17.6	23.3
Citizens' responsibility	8.9	7.9
Prewar	6.2	13.0
Postwar	11.8	2.3
N, both generations	79	87
Prewar	45	44
Postwar	34	43

Note: Entries are percentages mentioning one or more democratic component. Multiple responses were allowed. See the Appendix for details on the question wording and recodes.

TABLE 3

Support for Direct Democracy Procedures

	EAST		WEST		
REFERENDUM INITIATED	BERLIN	VOLKSKAMMER	BERLIN	BUNDESTAG	GAMMA (BERLIN)
A. By the parliament and legally nonbinding	35.4	35.0	18.0	12.0	.41
B. By the executive and legally nonbinding	19.0	22.0	10.1	5.0	.31
C. By a minority in parliament	49.4	26.0	24.7	10.0	.45
D. By a majority in parliament	60.8	67.0	33.7	16.0	.46
E. By the executive	45.6	35.0	19.1	6.0	.51
F. By the people	78.5	71.0	64.0	40.0	.34
Support C–F	32.0	[a]	9.0	[a]	.66

Sources: Berlin data is from the Berlin survey; Bundestag data is from Herzog et al. 1990, 130; Volkskammer data is from Werner 1991, 431.
[a] Entries are not available.
Note: Entries are percentages viewing each procedure as meaningful. See the Appendix for question wording and exact response categories. N = 79 for East Berlin, 87 for West Berlin.

or providing social security for everybody ought to be an important obligation of a democracy. In contrast, only 7.9% in the West mention this component. Again, the substantial generational differences within the East attest to the fact that while all East German elites were exposed to socialist regime messages, postwar elites were particularly receptive to them (hypothesis 4).

Differences between East and West MPs also emerge over the degree to which they support the use of plebiscitarian procedures. About a quarter (25.3%) of MPs from the East mention at least one direct-democracy procedure (e.g., referenda for all important decisions) whereas only 2.2% in the West mention such element. Within the East, the generational differences attest to the cumulative learning of individuals: both pre- and postwar generations are influenced by the people's revolution and the socialist emphasis on direct democracy, but younger elites who are less resistant to new experiences than older ones, are more strongly influenced by these factors (hypothesis 6).

Within the West, the weak generational differences regarding the plebiscitarian procedures are surprising, especially in light of the postmaterialism literature (Inglehart 1990). One possible explanation for the absence of generational effects may be seen in the powerful socialization effect on MPs of representative institutions, which leads even those MPs who initially favor plebiscitarian procedures to abandon these views as a result of their parliamentary work. As one Green parliamentarian put it when direct democratic procedures were discussed during the interview: "If you had asked me this ten years earlier, I would have been for it." This MP then discussed how his/her initial enthusiasm for plebiscitarian procedures faded as (s)he became confronted with "unreasonable" demands from the constituency. Although this must remain a tentative attempt to explain this unexpected finding, it is also consistent with the result that more West Germans (60.7%) than East Germans (40.5%), including the postwar cohort, mention the existent (representative) institutions of the West German constitution (elections, majority rule, etc.). Combined, the Green MP's conversion and the general focus of West elites on representative institutions point to the ability of these institutions to imbue all MPs with representative-democratic values.

East MPs' support for plebiscitarian procedures is equally pronounced when MPs evaluate a series of closed-ended questions about citizens' direct involvement in politics. Since these items were also asked in two surveys of the West German Bundestag and the East German Volkskammer, I may also compare Berlin MPs with national-level MPs (Table 3). The first two procedures are nonbinding referenda, whereas procedures C–F would oblige legislators to act in accordance with the outcome of a referendum. East MPs on the national level and in Berlin are considerably more likely to support plebiscitarian procedures, especially when the procedures obtain legally binding status. In fact, East Berlin elites frequently accompanied their rejection of the first two options with comments indicating their disagreement with the non-binding nature of these procedures. The proportion of elites supporting all four procedures (32.9% in East Berlin, 9.0% in West Berlin) reflects the different democratic visions that exist across the East–West boundary: in the West, plebiscitarian procedures are frequently seen as undermining democratic systems, whereas in the East, they are often viewed as a potential corrective to a detached political elite.[11]

The similarity of the Berlin and national results in terms of the plebiscitarian procedures also provides partial evidence that the Berlin results tend to be generalizable to the national political elite. For the East–West differences are, if anything, even larger on the national level, at least for these indicators. No doubt, one would need a national survey, paralleling the Berlin study, in order to establish with full confidence the generalizability of the Berlin-based results. But in the absence of such a study, the similarities presented are persuasive.

Taken together, the analysis of East and West MPs' conceptions of democratic values reveals a different mix of democratic components. While all MPs frequently mention civil rights and political competition, a substantial proportion of East MPs primarily value direct democracy and social equality elements, whereas West MPs predominately specify (representative) institutions and competition among political and social groups. The differences emerge particularly between postwar elites, suggesting that both East and West German institutions and the peoples' revolution of 1989 molded elites' values. Thus both East and West elites support liberal democratic rights, but this commitment, while similar on the surface, is embedded in different democratic ideals.

SOURCES OF VALUE PREDISPOSITIONS: A MULTIVARIATE ANALYSIS

How well does the influence of institutional learning perform as a predictor in a multivariate analysis when other individual-level East– West differences are considered simultaneously? Three dependent variables are analyzed: a democratic-rights index based on the indicators from Table 1, an egalitarian democracy indicator based on an additive index of responses to the open-ended democracy question, and a direct-democracy indicator based on the items from Table 3. (The Appendix contains details on the construction of the indicators.) I use these three different measures in order to show that the results converge regardless of the indicator used. Further, the democratic rights and plebiscitarian indicators have been employed in previous studies so that I am able to compare previous results with the ones presented here.

An East–West indicator measures the direct effect of institutional learning on elites' democratic values. If this variable is a significant predictor of MPs' conceptions of democracy, it would signify the importance of regime experience independent of other East–West differences on the individual-level. Communist party membership is also employed to measure the influence of institutional learning on elites' democratic values. Presumably, communist party members were exposed more intensely to the institutional norms of the socialist system than other MPs (Dalton 1994).[12]

It is also well-documented that individuals' religiosity tends to generate center-conservative political orientations, such as conservative economic or cultural issue positions (Lipset and Rokkan 1967, intro). Since religion is considerably more important in West than in East Germany, one must include this characteristic in the model. Further, elites' material–postmaterial value priorities are included because postmaterialists are more likely than materialists to support an increase of citizens' involvement in politics, favor broad citizen participation in politics, and (though to a lesser extent) sympathize with an egalitarian distribution of economic goods. Since East Germans are less postmaterialist than West Germans, one must control for these value orientations in examining the influence of institutional learning on democratic values.

In addition to MPs' cohort membership, MPs' education must be included because education, like generation, reflects elites' regime experience (Weil 1985). Further, individuals with lower incomes may endorse egalitarian democratic ideals in order to improve their personal situation. Thus East German elites may support an egalitarian democracy because of their economic difficulties, not necessarily because of their regime experience. Finally, I analyze the potential impact of respondents' gender on their ideological predispositions because the East German state facilitated womens' efforts to join the workforce by providing, for example, access to day-care centers (Rueschemeyer and Lemke 1989). Therefore, female MPs from East Germany may endorse egalitarian democratic principles because of the perceived difficulties in pursuing a career path under the West German system, not because they have learned to endorse principally a broad range of egalitarian ideals.

Table 4 displays the results from an ordinary least squares analysis. For each dependent variable, I analyzed the equations separately for the East and the West in order to uncover potential differences of the impact of predictor variables. Then I applied the equation to the combined data in order to assess the impact of the systemic variable (controlling for individual-level variables) on elites' conceptions of democracies.

Several results are noteworthy. First, the regime membership coefficient is highly significant for the democratic rights index (beta = .17). Independent of East–West variations in sociodemographics, postmaterialist values, or religiosity, institutional learning influences moderately, though not dramatically, elites' support for general democratic norms (hypothesis 1). Second, the Communist party coefficient (beta = −.12), while statistically insignificant, still suggests that Communist party MPs (PDS) are slightly less likely than other MPs to endorse general democratic rights, a result that parallels the findings from a public opinion study in East and West Germany (Dalton 1994). Finally, elites' postmaterialist values also generate support for democratic rights, both in the East and the West. Unexpectedly (contra hypothesis 2), the postwar generation among East German elites is substantially more, not less, likely to support democratic rights than the prewar generation (beta = .24). The strength of the coefficient is particularly impressive in light of the fact that sociodemographic variables have in the past failed to predict elites' core values. (An explanation will be provided shortly). Moreover, the generational variable is also insignificant for West elites. Apparently, prewar elites in the West have been sufficiently socialized by democratic institutions so that any cohort differences over democratic rights that initially might have existed disappeared.

Parliament members' egalitarian democratic conceptions are strongly influenced by the process of institutional learning. The strength of the regime

TABLE 4

Predicting MPs' Democratic Values

INDEPENDENT VARIABLES	DEMOCRATIC RIGHTS			EGALITARIAN DEMOCRACY			DIRECT DEMOCRACY		
	EAST	WEST	COMBINED	EAST	WEST	COMBINED	EAST	WEST	COMBINED
Regime experience[a]	—	—	.17** (1.8)	—	—	−.30*** (−.41)	—	—	−.28*** (−1.5)
Communist party	−.12 (−1.4)	—	−.01 (−.21)	.25* (.53)	—	.21*** (.47)	.21 (1.4)	—	.13 (1.2)
Postwar generation	.26** (2.6)	.04 (.46)	.13* (1.4)	.08 (.15)	−.13 (−.07)	.04 (.05)	.05 (.25)	.08 (.38)	.06 (.34)
Postmaterialism	.41*** (2.4)	.30*** (1.8)	.35*** (1.9)	.01 (.01)	.17 (.04)	.07 (.05)	.18 (.56)	.06 (.14)	.11 (.32)
Religiosity	−.18 (−.60)	−.03 (−.11)	−.05 (−.16)	−.12 (−.07)	−.01 (−.01)	−.06 (−.03)	−.01 (−.01)	−.20* (−.32)	−.14 (−.19)
Education	.13 (.77)	.32*** (1.4)	.24*** (1.2)	−.12 (−.13)	.05 (.03)	.06 (.04)	−.08 (−.29)	−.08 (−.17)	−.10 (−.25)
Income	.08 (.45)	−.12 (−.62)	−.02 (−.10)	−.01 (−.01)	.06 (.01)	−.01 (−.01)	−.02 (−.07)	−.02 (−.04)	−.01 (−.01)
Gender[b]	.10 (1.0)	−.04 (−.51)	.02 (.26)	−.17 (−.34)	−.12 (−.07)	−.14 (−.21)	−.15 (−.87)	−.11 (−.56)	−.11 (−.68)
Adj. R^2	.19	.18	.20	.03	.01	.17	.09	.04	.17
N	76	85	161	76	85	161	76	85	161

Note: Entries are standardized regression coefficients (OLS); unstandardized coefficients appear in parenthesis. The democratic rights index is an additive index based on items presented in Table 1. (The Appendix contains details about the construction of the indicators.) The egalitarian indicator is an additive indicator of the number of egalitarian responses to the open-ended democracy question. The direct-democracy index is an additive index based on items presented in Table 3.

[a] High = west.

[b] High = male.

*$p \leq .10$.

**$p \leq .05$.

***$p \leq .01$.

membership indicator (beta = −.30) is remarkable, considering that several individual-level characteristics are included in this analysis. Further, as expected, communist party MPs are more likely than other MPs to support egalitarian democratic conceptions. And while the generational coefficient is insignificant within the East, the total effect of the postwar generation (r = .21) still suggests that postwar MPs are more likely than prewar MPs to endorse egalitarian democratic ideals.

The analyses of elites' evaluations of direct-democracy procedures confirm that East elites are more likely than West elites to endorse direct-democracy conceptions (beta = −.28). This unequivocal result is particularly noteworthy because the model includes a number of predictors typically considered to be important individual-level sources of favorable views on plebiscitarian views, especially postmaterialism. Furthermore, although the cohort coefficient is insignificant, substantial cohort effects appear within the East when I use a direct-democracy indicator based on the open-ended question[13] (beta = .26) but still not within the West (beta = .03). Within the East, this suggests that prewar and, especially, postwar cohorts are supportive of plebiscitarian procedures. Within the West, this finding is consistent with the account given by the MP quoted earlier: MPs' initial support for direct democracy procedures, which is typically concentrated among postwar cohorts, may weaken as a results of MPs' involvement in parliamentary work. Finally, there is no ready explanation for the weak effect of postmaterialism (beta = .11), especially in light of the large number of studies establishing the link between postmaterialism and direct-democracy procedures (Inglehart 1990).[14]

On the whole, these analyses attest to the importance of regime experience as an important source of elites' conceptualization of an ideal-typical democracy. Substantial East–West differences exist over the social egalitarian and plebiscitarian dimension, corroborating the institutional-learning argument. In addition, several (although not all) cohort differences within the East and the West are consistent with the notion of cohort-based culture change, particularly for the social egalitarian and plebiscitarian dimension.

Value Divergence Versus Value Convergence

However, the East–West gap is less pronounced in terms of elites' commitment to liberal democratic rights, especially in the context of the open-ended question. In addition, the fact that within the East, the postwar generation is substantially more supportive of general democratic rights than the prewar generation cannot be explained with the institutional learning argument. The substantial support for liberal democratic rights among East elites converges with findings from studies of recent regime transitions in Europe, namely, that support for liberal democratic rights is surprisingly strong in light of these nations' lack of democratic experience. In order to account for this finding, several analysts of democratic transitions have resorted to a value diffusion model in explaining the evolution of democratic norms in Southern and East-Central Europe (Di Palma 1990; Fuchs, Klingemann, and Schoebel 1991; McDonough, Barnes, and Lopez-Pina 1986; Pye 1990; Starr 1991). The central premise of this perspective is that a variety of domestic and international factors (e.g., the mass media, economic development, education) enables citizens to compare their authoritarian system with the alternative offered by a democratic political system (Weil 1993, chap. 9). Since liberal democracy prospered economically and politically in the postwar era in Western Europe, many citizens in Southern and East-Central Europe endorse the liberal democratic principles that are associated with the promise and prestige offered by a democracy. Since East German citizens could receive West German television, which undoubtedly helped to erode the legitimacy of the East German economic and political system, this process may have contributed to the evolution of support for liberal democratic rights among East elites.

I would like to stress that I limit the diffusion argument to the evolution of general democratic rights. I do not wish to argue that East Germans have become complete democrats who, in addition to supporting liberal democratic rights, possess the ability to act democratically. For example, opinion research has frequently documented that while general rights are easily endorsed, these rights are frequently not applied to specific minority groups (Duch and Gibson 1992; Sullivan, Piereson, and Marcus 1982).[15] Still, if East Germans express an abstract preference for general democratic rights, it would indicate that the initial conditions for reconstructing a democratic system are more favorable in contemporary Germany than they were in West Germany in the immediate postwar years (Merritt and Merritt 1970; Weil 1989).

In order to conduct an indirect test for the presence of diffusion effects, consider that the diffusion perspective, combined with the notion of generational change, suggests that the democratic values of East and West elites are "pushed" toward convergence. Since the onset of the diffusion process falls into the postwar era (Pye 1990), its force should be primarily felt among the postwar generation of East MPs. Therefore, one indication for the influence of value diffusion would be if East–West differences over democratic rights are smaller within the *postwar* than within the prewar cohort. In contrast, the institutional learning perspective, combined with the notion of generational change, predicts that East and West institutions produced differences *especially* among the postwar cohort. These predictions cannot be tested directly by the analyses in Table 4 because the regime experience variable is absent (columns 1–2) or the analyses do not distinguish directly between prewar and postwar generation (the combined analyses). Instead, I reanalyzed the sources of elites' values within the prewar and postwar generation, paying special attention to the strength of the East–West indicator across cohorts.

The results indicate that postwar elites' support for liberal democratic rights is partly influenced by converging forces (Table 5). For example, the regime membership indicator is insignificant for postwar elites' (b = .49). Instead, elites' education and, especially, postmaterialist values are the best predictors of elites' democratic values. Hence, postwar elites in East and West Germany are primarily divided by postmaterial values and educational background but not by their regime experience—at least as far as their commitment to liberal democratic values is concerned. In contrast, the regime experience variable is highly significant within the prewar cohort (b = 3.2), suggesting that the effects of value diffusion are smaller among prewar than postwar elites.

Yet the results for the egalitarian ideals equation also document the limits of the diffusion argument. In particular, the regime variable is especially strong among postwar cohorts (b = −.56), producing substantial East–West differences over social egalitarian ideals among the postwar elites. In contrast, the regime experience variable is only weakly related to prewar elites' egalitarian values (b = −.22). On this dimension, then, East and West elites have drifted apart during the time of division.

Finally, the results for the direct-democracy indicator provide the strongest evidence yet of the support for plebiscitarian procedures among all East elites. The most important predictor is elites' regime membership, for both prewar (b = −1.4) and postwar (beta = −1.5) elites. Individual-level traits, such as postmaterial values, are relatively unimportant in explaining elites' support for citizens' direct involvement in politics—undoubtedly a result of the significant role that East German citizens played in the overthrow of the East German regime and the socialist emphasis on direct democracy.

On the whole, these results support the interpretation that support for liberal democratic rights has been diffused into East Germany. At the same time, the diffusion of ideal-typical conceptions of democracies has not occurred to the same extent. Unlike their West German counterparts, East German postwar elites especially value liberal democratic rights in combination with social egalitarian and plebiscitarian ideals.

CONCLUSION

The unique historical situation of Germany's division and unification enables analysts to examine the insti-

TABLE 5

Predicting MPs' Democratic Values, by Generation

INDEPENDENT VARIABLES	DEMOCRATIC RIGHTS PRE-WAR	DEMOCRATIC RIGHTS POST-WAR	DEMOCRATIC RIGHTS COM-BINED	EGALITARIAN DEMOCRACY PRE-WAR	EGALITARIAN DEMOCRACY POST-WAR	EGALITARIAN DEMOCRACY COM-BINED	DIRECT DEMOCRACY PRE-WAR	DIRECT DEMOCRACY POST-WAR	DIRECT DEMOCRACY COM-BINED
Regime experience[a]	.30*** (3.2)	.05 (.49)	.17** (1.8)	−.19 (−.22)	−.35*** (−.56)	−.30*** (−.41)	−.29*** (−1.4)	−.25* (−1.5)	−.28*** (−1.5)
Communist party	−.03 (−.80)	−.10 (−1.4)	−.01 (−.21)	.10 (.10)	.25* (.54)	.21*** (.47)	.17 (2.0)	.14 (1.1)	.13 (1.2)
Postwar generation	—	—	.13* (1.4)	—	—	.04 (.05)	—	—	.06 (.34)
Postmaterialism	.26*** (1.5)	.49*** (3.2)	.35*** (1.9)	.09 (.05)	.08 (.07)	.07 (.05)	.17 (.45)	.07 (.23)	.11 (.32)
Religiosity	.04 (.13)	−.15 (−.53)	−.05 (−.16)	−.05 (−.02)	−.17 (−.09)	−.06 (−.03)	−.19* (−.31)	−.01 (−.01)	−.14 (−.19)
Education	.18* (.93)	.32*** (1.4)	.24*** (1.2)	−.01 (−.01)	−.10 (−.07)	.06 (.04)	−.14* (−.32)	−.09 (−.22)	−.10 (−.25)
Income	.10 (.47)	−.07 (−.30)	−.02 (−.10)	−.16 (−.08)	.10 (.08)	−.01 (−.01)	−.05 (−.11)	−.03 (−.08)	−.01 (−.01)
Gender[b]	.16 (2.1)	−.03 (−.07)	.02 (.26)	−.14 (−.19)	−.16 (−.27)	−.14 (−.21)	−.11 (−.67)	−.16 (−.97)	−.11 (−.68)
Adj. R^2	.08	.31	.20	.08	.21	.17	.20	.10	.17
N	87	75	161	86	75	161	86	75	161

Note: Entries are standardized regression coefficients (OLS); unstandardized coefficients appear in parenthesis. The democratic rights index is an additive index based on items presented in Table 1. (The Appendix contains details about the construction of the indicators.) The egalitarian indicator is an additive indicator of the number of egalitarian responses to the open-ended democracy question. The direct-democracy index is an additive index based on items presented in Table 3.

[a]High = west.

[b]High = male.

*$p \le .10$.

**$p \le .05$.

***$p \le .01$.

tution–culture linkage under quasi-experimental conditions. Consistent with systemic political culture studies that stress institutional learning as a source of elites' values, East and West elites' democratic conceptions are substantially shaped by individuals' exposure to a socialist or liberal democratic political system. The strength of regime experience as a predictor of elites' ideological values is remarkable because the empirical models include a number of individual-level variables (esp. cohort membership) that partially absorb the influence of elites' regime experience on their ideological values. Thus both East and West German systems were partially successful in reshaping the political culture. But the convergence of postwar cohorts over general democratic rights also attests to the diffusion of these rights into East Germany.

The partial success of the East German state's efforts to imbue its citizens with social egalitarian and plebiscitarian ideals suggests that the collapse of a specific set of institutions does not necessarily indicate the absence of institutional learning. The East German state collapsed, but a substantial number of East MPs continues to value socialist–democratic ideals. More generally, whether or not the collapsed institutions in East-Central Europe have imbued citizens with regime-conforming values must be examined in empirical studies and cannot be inferred from the collapse of socialist institutions itself.

Furthermore, the existence of partially different conceptions of democracies in East and West Germany also underlines the need to be cautious in applying Western (liberal–representative) democratic principles to the newly emerging democracies in East-Central Europe (Collier and Mahon 1993). Before analysts are able to specify the notion of democracy properly in the context of democratic transitions, they must know elites' conceptions of democracy in East-Central Europe. These considerations then call for further analyses of political elites' ideal-typical democratic conceptions in East-Central Europe.

The results also indicate the difficulty of merging Germany into a truly united country. While the power distribution among the East and West undoubtedly ensures that East elites have less influence on major political decisions than West elites, the value differences nevertheless suggest that East elites' preferences will differ from West elites' preferences on a number of policy domains as well as on more fundamental issues (e.g., plebiscitarian procedures). Hence, the legal unification and the value differences between East and West German elites raises once again the possibility of severe political and social tensions in Germany.

On a more optimistic note, the onset of a value convergence in terms of liberal democratic rights also suggests that the issues facing Germany today are partially different from those which West Germany faced in the immediate World War II years. At that time, the major task was to convince the non- or antidemocratic citizens of the virtues of a democratic political system. But the presence of a general commitment to democratic rights in East Germany suggests that at least in principle (if not in practice), East German elites do not need to be persuaded of the virtues of a democratic system—they already are. Rather, one new task is to convince a significant portion of East German elites of the virtues of a *parliamentary–representative* democracy. What is new, then, is that a debate may take place over which democratic system is the most appropriate for the united Germany. In this sense, the unification of Germany not only revives old, troubling, issues but may also add a new, perhaps less destructive, dimension to the vexing course of Germany's history.

APPENDIX: COLLECTING THE DATA AND MEASUREMENT ISSUES

Several steps were undertaken to maximize the cooperation of Berlin MPs. Before the actual interviewing started in October 1991, an introductory letter was sent to all MPs in the summer of 1991. The letter introduced the project and announced the intention to meet personally with all legislators. I also met all party leaders who agreed to support the study. For example, party leaders agreed to make an announcement in a parliamentary group meeting asking MPs of their respective party to participate in the project. Simultaneously with the announcements, a second letter was sent to MPs, this time requesting a specific interview time. Although relatively few MPs responded to this request promptly, all MPs at this time had become familiar with the project. Most of the interviews were actually scheduled during the biweekly parliamentary session, which enabled me to contact personally those MPs who had not responded to the previous requests.

A total of 168 MPs was interviewed. The cooperation of MPs was excellent. Smaller parties are somewhat overrepresented because the almost intimate atmosphere among a small parliamentary group of, say, 15 to 20 MPs substantially increased the visibility of the project. (A table comparing the partisanship of interviewed MPs with the entire parliament is available upon request.) Extensive comparisons of the data with the entire parliament suggest that the data closely mirrors the parliament on important characteristics, such as age, education, gender, occupation of parliamentary leadership positions, and seniority.

A first draft of the questionnaire underwent close scrutiny before the field work actually began. In October 1991, two party leaders from local district parliaments from East and West Berlin participated in a formal pretest of the questionnaire. Both leaders also discussed the questionnaire at length.

Measurement and Coding Schemes. The responses to the open-ended democracy question were coded based on detailed notes taken during the interview. I used a slightly modified coding scheme developed by Putnam (1973, 270–73).

1–8. The term democracy is frequently used without further specifications these days. What seem to you personally the essentials of a democracy? {CODE UP TO FIVE MENTIONS}.

Government by the People

11. Government by the people; popular control; control by the people
12. Popular interest in and awareness of politics
13. Responsibility or answerability of the government to the people; government by consent; government based on electoral mandate
14. Popular participation; an active role of the people; popular involvement in decision making; direct democracy within the framework of parliamentary democracy
15. Direct democracy; referenda for important decisions; public should be able to recall ministers at any time;
19. Other

Equality and Social Democracy

21. Equality in general
22. Political equality; one man, one vote
23. Equality of opportunity; each person has the possibility of developing him/herself as far as possible; participation of citizens in all areas of society
24. Just standard of living; freedom from want; social and economic security for all
25. Classless society; less social distance; fewer rich and poor; less social privilege
26. Social ownership/control over the economy; industrial democracy
27. Provide minimum social security/right to a job
28. Gender equality
29. Other

Liberty

31. Liberty; freedom in general
32. Political or civic liberties in general
33. Freedom of expression (speech, free press)
34. Minority rights; consideration of the minority
35. Limited government; checks and balances; no arbitrary power
36. Laissez-faire, socially and economically. Freedom from government interference in socioeconomic affairs
37. Religious liberty
39. Other

Institutions and Procedures

41. Elections
42. Majority rule
43. Representative or parliamentary government in general
44. Parliamentary or legislative control over the executive
45. Rule of law; legal due process (*Rechtsstaat)*
46. Defendable democracy
49. Other

Political Competition and Choice

51. Possibility of government changes; minority can become majority
52. Party competition; more than one party
53. Strong, critical opposition
54. Elite competition; ruling oligarchy
59. Other

Societal Conditions

61. Pluralism, variety of private associations and institutions
62. Consultation by the government with groups and organizations
63. Parties as centers of participation and agents of representation
64. Absence of party discipline
65. Decentralized institutions; federalism
66. Ecologically sound politics
69. Other

Characteristics of Citizens

71. Mature, educated, intelligent citizens
72. Freedom to do what is right; individual self-control
73. Assumption of responsibility and duties
74. Action in the interest of collective, not only of individual
75. Reciprocal respect and tolerance
79. Other

Other

81.
98. Don't know
99. Not answered

The recodes for the data presented in Table 2 are as follows: government by the people (11–13), active participation (14), direct democracy (15), social equality (24–27), Equality of opportunity (21–23), civil rights/limited government (31–36), institutions (41–45), political competition (52–54), societal competition (61–62), citizens' responsibility (71–75).

Democratic Rights. Members of parliament were shown a card containing the indicators from Table 1 and asked, "Would you tell me for each of the statements, whether you agree or disagree?" The card also displays a seven-point scale, defining the polar ends of *agree completely* (7) and *disagree completely* (1). Percentages in Table 1 are based upon MPs who agree with a statement (7–5). The indicator used in the multivariate analyses is additive after all items have been recoded such that agreement with a statement reflects democratic attitudes. Cronbach's alpha for the East, West, and combined data is .61, .63, and .62, respectively. The variable's minimum equals 20, maximum 42, mean 32.6, and standard deviation 5.4.

Equalitarian Democracy. I created an additive index of all the egalitarian responses to the open-ended democracy question (24–27 in Table A-1). The variable's minimum equals 0, maximum 4, mean .32, and standard deviation .69. Given the skewed distribution of this variable, particularly in the West, I also analyzed this model using probit, but the results generally lead to the same substantive conclusions, with two exceptions. Within the East, the Communist party coefficient misses statistical significance by a small margin (p = .11) and the gender coefficient is significant: female MPs are somewhat more likely to mention egalitarian democratic components.

Direct Democracy. For the direct-democracy indicators (Table 3), I used a dichotomous response format in order to keep the results comparable to the study of national-level East and West German MPs (Herzog et al. 1990; Werner 1991). The question reads, "Would you please indicate your opinion on the plebiscitarian involvement of citizens by indicating whether you find each of these procedures involving citizens directly meaningful or not meaningful?" Responses in Table 3 are respondents who find the procedure meaningful. For the plebiscitarian index used in multivariate analyses, I first included the small number of missing values as a neutral middle category and then created an additive index. Cronbach's alpha in the East, West, and combined data is .71, .65, and .71, respectively. The variable's minimum equals 4, maximum 12, mean 7.9, and standard deviation 2.7.

Regime Membership. This dichotomous variable is coded 0 for East Berlin MPs and 1 for West Berlin MPs.

Communist Party. This dichotomous variable is coded 1 if the MP is a member of the reformed Communist party (PDS) and 0 if not.

Postwar Generation. In this dichotomous variable, 1 represents MPs born in 1945 or later, and 0 represents MPs born in 1944 and earlier. I chose this cutting point for generations because it is theoretically meaningful *and* because it leaves enough cases within each cohort to conduct the cohort analyses. The decision to divide MPs into two groups—those born in 1944 or before and those born later—means that MPs reaching the age of 15 before 1960 are coded as a prewar cohort. This cutting point parallels the degree to which the quasi-laboratory conditions had been established in Germany because the Berlin Wall was not built until 1961. Before that time, East and West Germany were less insulated from each other. I have conducted a sensitivity analysis using 11 different cutting points for the cohort definition. For example, I first defined a prewar cohort by combining MPs born in 1938 and before into the prewar category. A second prewar variable includes those born in 1939 and before, a third contains MPs born in 1940 and before, and so on. I then conducted the analyses from Table 4 and 5, using these different cohort variables in separate analyses. The results parallel the historical reality in Germany: the largest cohort differences within the East emerge when the postwar cohort predominately contains MPs who grew up after the construction of the Berlin Wall. While a more refined breakdown of cohorts would undoubtedly be desirable, these analyses suggest that the cohort analyses presented here are robust and do not depend on an arbitrary birth date cutting point.

Postmaterialism. I used a slightly modified coding procedure of the original items Inglehart developed. While materialists and postmaterialists are coded as Inglehart suggests, I distinguish between mixed–materialists and mixed–postmaterialists, depending on whether respondents first mention the materialist or the postmaterialist item. This coding procedure uses more information than the three-point indicator. (The results are almost identical when the mixed categories are combined.) The variable's minimum equals 1, maximum 4, mean 3.2, and standard deviation .94.

Religiosity. "How often do you attend church? Would you say: At least every Sunday; almost every Sunday; sometimes; once a year; less than once a year; never." High values represent religious responses. The variable's minimum equals 1, maximum 6, mean 4.3, standard deviation 1.5.

Education. Higher values reflect higher educational attainment. (1) Volkschule/8. Klasse, (2) Mittlere Reife/10. Klasse/Mittel Schule, (3) Abitur/EOS, (4) Fachhochschule/Fachschule, (5) Universitaet. The variable's minimum equals 1, maximum 5, mean 4.3, standard deviation 1.1.

Income. (1) Below 4,000DM, (2) 4,000–5,999DM, (3) 6,000–7,999DM, (4) 8,000–9,999DM, (5) above 10,000DM. The variable's minimum equals 1, maximum 7, mean 2.7, standard deviation 1.1.

Gender. This is coded 1 for male MPs and 0 for female MPs.

Notes

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1. In order to avoid the repetitive use of one term, I use the terms *conceptions of democracy, core values,* and *ideological values* interchangeably.

2. Other elite studies document the relatively weak influence of sociodemographics on elites' attitudes (Beyme 1971; Crewe 1974; Edinger and Searing 1967; Herzog 1975).

3. Additional systemic factors mentioned are the socioeconomic composition of elite sectors when modern political systems formed and the outcome of political competitions between social groups.

4. Empirical studies of mass political culture try to circumvent the case problem by collecting institutional data and information about political cultures from public opinion surveys of a large number of countries (Bollen and Jackman 1989; Inglehart 1988; Weil 1989). Although valuable, these studies must rely on relatively few indicators of mass political culture. Other studies, in turn, examine the evolving democratic attitudes of mass publics in Spain (McDonough, Barnes, and Lopez-Pina 1986) or Germany (Baker, Dalton, and Hildebrandt 1981); or else they examine the link between regional institutions and regional cultures (Putnam 1993). But none of the cases examined offers the opportunity to compare directly the influence of two fundamentally opposed systems on elites' values.

5. Analyses of mass political cultures in other Central Europe nations also appear to corroborate this argument. For example, Almond (1983) notes that the Polish mass public began to endorse egalitarian values, like the Czech public (McGregor 1991). Similarly, a recent study of mass publics in the "republics" of the former Soviet Union notes that there has not been a "wholesale abandonment of socialist principles" (Finifter and Mickiewicz 1992, 861).

6. Indeed, previous studies of mass publics attest to the importance of institutional arrangements (Powell 1986) and individuals' group membership (Klingemann 1979) as sources of political orientations and behavior.

7. In this discussion, I assume that the new political system is as alien to the preexisting political culture as the respective socialist and democratic systems were in Central Europe and West Germany after World War II and as democratic systems are now in contemporary Central Europe and in East Germany. Consequently, a different argument would have to be constructed for cases like the United States or Great Britain, where the gradual evolution of democratic political institutions over centuries makes it virtually impossible to disentangle discrete variables representing the effect of institutions on culture and vice versa.

8. Consistent with this perspective, cohort studies in West Germany find that elites' historical experiences during their formative socialization years markedly affects the predominant values of each generation (Baker, Dalton, and Hildebrandt 1981). Analogously, postwar elites in Central Europe, who only experienced the socialist systems, should be shaped more by socialist values and practices than prewar elites, whose cohort experience also includes nonsocialist systems.

9. These variations are largely controlled for because many, although not all, MPs grew up in Berlin.

10. A question about MPs' background revealed that two West Berlin MPs escaped from the GDR after they passed adolescence. I excluded them from the study.

11. This interpretation about the reasoning of MPs is based on a preliminary analysis of an open-ended question asking whether MPs would support solving the asylum issue through a referendum. A detailed discussion cannot be pursued here.

12. Throughout the field work, I tried to obtain additional information about East MPs' involvement in various GDR organizations. It turned out to be extraordinarily difficult to find this information in archives. In addition, the published biographies of East MPs frequently do not mention membership in GDR organizations. Although a more complete mea-

sure of the extent of regime involvement would undoubtedly be desirable, this information could not be found.

13. This alternative indicator is an additive indicator of the number of mentions of categories 14 and 15 in the Appendix.

14. It should be studied in future elite surveys whether this finding is unique to the Berlin study or whether institutional learning mitigates the predispositional linkages between postmaterialism and direct democracy on the level of elites. In the absence of comparable elite surveys, this issue must remain unresolved here.

15. I have presented evidence elsewhere showing East MPs are substantially less tolerant than West MPs, despite the strong support for liberal democratic rights among East MPs (Rohrschneider 1993).

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CONTROVERSY

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QUESTION FORM AND CONTEXT EFFECTS IN THE MEASUREMENT OF PARTISANSHIP: EXPERIMENTAL TESTS OF THE ARTIFACT HYPOTHESIS

Previous articles in this Review, *including a Controversy in 1992, debated the comparability of alternative forms of the question about partisanship asked in Gallup and Michigan SRC surveys. Bishop, Tuchfarber and Smith contribute to this debate by reporting and analyzing evidence from 15 experimental surveys in Ohio in 1991–1993. They conclude that the distribution of partisan loyalties will generally be the same whether one uses the Gallup or Michigan Survey Research Center question and that, contrary to findings of Abramson and Ostrom, the Gallup form is no more responsive to short-term political forces than its SRC counterpart. In response, Abramson and Ostrom agree that during many time periods there will be little difference between aggregate levels of macropartisanship regardless of which measure is used. But they argue that during periods of political volatility the Gallup approach will accentuate differences, while the SRC version will attenuate them.*

COMMENT

Political party identification has become probably the most widely used theoretical construct in the literature on American political behavior.[1] Though its measurement dates back to at least the early days of public opinion polling in the 1930s, and even a bit before that, it was not until the 1950s that it became established at the Michigan Survey Research Center (SRC) as *the* intervening variable in the study of the American voter.[2] While it has become a well-established staple in the empirical study of American politics, various investigators have raised questions from time to time about the dimensionality, measurement, and meaning of party identification.[3] More recently, political scientists and public opinion pollsters alike have also raised significant doubts about the comparability of the two most widely used measures of partisanship in the literature: the Gallup form, "In politics, as of today, do you consider yourself a Republican, a Democrat, or an Independent?," and the Michigan SRC form, "Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent, or what?" (Abramson and Ostrom, 1991, 1994; Bishop, Tuchfarber, and Smith 1992, 1993; Kagay 1991; Kohut 1991; MacKuen, et al., 1992; see also the anticipation of this controversy by Converse and Pierce 1985).

The Gallup form of the question, it is typically argued, is more responsive to the effects of short-term economic and political forces because it focuses the respondent's attention on how he or she feels about the two political parties "as of today," whereas the Michigan form is said to elicit more of the respondent's long-term loyalty to one of the two parties with its emphasis on how he or she "generally" and "usually" thinks. It is claimed, therefore, that any significant difference (1) between the marginal distributions of party identification produced by these two question forms at any one point in time, (2) in the variability of responses to these two forms over time, or (3) in their intercorrelations with various short-term indicators (e.g., presidential approval, consumer sentiment, voting preferences) results from the difference in *time horizon* created by these alternative wordings of the question (Abramson and Ostrom 1991, 1994; MacKuen et al. 1992, resp.; see also Borrelli, Lockerbie, and Niemi 1987 and Fine 1992).

THE MACROPARTISANSHIP CONTROVERSY

This ongoing argument about the time frame in the wording of these two versions of the party identification question lies at the heart of the recent controversy in this *Review* about whether the revisionist findings on macropartisanship reported by MacKuen, Erickson, and Stimson (1989) can be generalized only to the Gallup, or also to the Michigan, form of the question (Abramson and Ostrom 1991, 1994; MacKuen et al. 1992, resp.) In their most recent response, MacKuen et al. (1992) maintain that their findings are not an artifact of the wording of the question used by Gallup, whereas Abramson and Ostrom (1994; MacKuen et al. 1992, resp.) adhere to the view that the bulk of the evidence supports their position that the wording of the question does make a significant and substantial difference in the conclusions one reaches about the changes, causes, and consequences of macropartisanship in the American electorate.

Until fairly recently, all of the evidence in this debate had come from nonexperimental observations of how the Gallup and Michigan measures of partisanship have fluctuated over time in response to such political indicators as presidential approval and to such economic barometers as consumer confidence. It soon became obvious to everyone involved in this controversy, however, that there was a critical need to test the *question-wording-artifact hypothesis* advanced by Abramson and Ostrom (1991, 1994; MacKuen et al.

1992, resp.) by comparing the Gallup and Michigan forms of the party identification question in a controlled experiment, using the same mode of interviewing within a single organization to rule out mode-of-interviewing effects and "house effects" (see Hugick 1991; Smith 1978). And to guard against ad hoc interpretations of mere noise, the findings, significant or not, needed be replicated under different political conditions and preferably with different populations.

EXPERIMENTS AND REPLICATIONS

In April 1991, we began a series of such experiments and replications, carried out as part of the Ohio Poll and the Greater Cincinnati Survey, all of which were conducted by computer-aided, random-digit-dialed telephone interviews at the University of Cincinnati's Institute for Policy Research.[4] In a previous paper, we presented the preliminary results from the first 7 experiments we did (Bishop, Tuchfarber, and Smith 1992). Here we will report the results of 8 additional replications (a total of 15 experiments altogether), 7 of which were done either during, or immediately after, the 1992 presidential election campaign.

In the interim we have become well aware of a similar set of experiments begun by Abramson and Ostrom (1994; MacKuen et al. 1992, resp.) in March 1992 in a series of surveys conducted in the state of Michigan.[5] As a result, where direct comparisons of evidence exist, we have contrasted their findings with ours. Pitting one set of results against another in the spirit of scientific exchange should be of benefit not only to the participants and spectators in this particular controversy but also to the intellectual growth of an "experimental political science" that Kinder and Palfrey (1993) have called for elsewhere.

HYPOTHESES

If the artifact hypothesis—or *time horizon hypothesis*, as we prefer to call it—is correct, we should find not only a significant difference between the marginal distributions of responses to the Gallup and Michigan forms of the question at any one point in time but also a significant variation in the degree to which each measure of partisanship correlates with a respondent's current political and economic attitudes on theoretically related, short-term indicators. If, as is argued, the Gallup form of the question is more sensitive to current events because of its *shortened time horizon* ("as of today"), then we should expect to find that responses to it are more highly and significantly correlated with responses to questions about the respondent's approval of the president's performance, confidence in the economy (consumer confidence), and current preferences for candidates for various political offices (and other short-term political evaluations) than are responses to the Michigan form of the question with its *extended time horizon* of "generally" and "usually" (see Abramson and Ostrom 1991, 1994; Kagay 1991; Kohut 1991; MacKuen et al. 1992, resp.) As we shall show, that is not the case.

FINDINGS

Rarely, if ever, do we have the opportunity to examine such a profusion of experimental evidence on an issue of such fundamental importance to the discipline as the measurement of political party identification. Let us look now at what the empirical evidence tells us.

Effects on Marginal Distributions

The results in Table 1 tell us that the wording of the party identification question made little or no difference in the distribution of responses. In 14 of the 15 experiments we conducted the difference was not statistically significant. The one exception that occurred in the Greater Cincinnati Survey of November–December 1991 is probably best regarded as due to chance since the rather odd pattern it produced (more Independents with the Gallup form and both more Democrats *and* Republicans with the Michigan form) does not appear consistently in the other 14 experiments. Indeed, finding a statistically significant difference in 1 out of 15 experiments is just about what would be expected by chance (at $p = .05$). Furthermore, this repeated finding of insignificant differences at different times, under different political conditions, and in three different populations, each with a somewhat distinct partisan coloration (the city, tending to be Democratic, the metro area, more Republican, the state, more evenly balanced) underscores the value of replication in this controversy in particular and in social science research more generally (see Neuliep 1991). It is not hard to imagine how a single survey with a large enough sample might generate a statistically significant difference (as in the one instance we observed) only to evaporate like "cold fusion" in the next independent replication (see Schuman and Presser 1981, 317–23).

Abramson and Ostrom (1994) have also found that the wording of the party identification question generally produces no significant difference in the *aggregate* distribution of responses. In only one of their six experimental comparisons did they find a difference that was statistically significant at the .05 level. For another replication of this result with multiple measures of party identification within the same survey, see Green and Schickler 1993. So on this point at least there appears to be substantial evidence and agreement among us that the wording of this question, in either the Gallup form or the Michigan SRC form, will generally make little or no difference in the conclusion a researcher would draw about the *aggregate* distribution of political party identification. That much seems settled.

TABLE 1

Political Party Identification by Question Form in Fifteen Experiments (%)

	QUESTION FORM		
SURVEY	GALLUP	MICHIGAN	X^2(df = 2)
Ohio Poll			
1. 24–30 Apr. 91			
Democrat	28.4	34.0	2.65, n.s.
Independent	42.2	40.5	
Republican	29.4	25.4	
Number of cases	313	338	
2. 2–11 Oct. 91			
Democrat	33.4	30.9	.78, n.s
Independent	39.5	39.1	
Republican	27.0	30.0	
Number of cases	296	317	
3. 20–30 Mar. 92			
Democrat	30.1	29.2	.54, n.s.
Independent	38.3	40.9	
Republican	31.6	29.9	
Number of cases	386	394	
4. 9–19 Sept. 92			
Democrat	33.3	31.0	.74, n.s.
Independent	38.6	39.1	
Republican	28.2	29.9	
Number of cases	412	481	
5. 16–25 Oct. 92			
Democrat	36.7	34.1	4.9, n.s.
Independent	38.1	33.8	
Republican	25.3	32.1	
Number of cases	431	414	
6. 28 Oct.–1 Nov. 92[a]			
Democrat	30.1	30.8	.07, n.s.
Independent	41.0	40.6	
Republican	28.9	28.6	
Number of cases	529	539	
7. 3–10 Feb. 93			
Democrat	33.3	34.2	.14, n.s.
Independent	40.0	40.2	
Republican	26.7	25.6	
Number of cases	408	398	
8. 3–14 May 93			
Democrat	27.1	30.0	1.2, n.s.
Independent	44.2	40.5	
Republican	28.7	29.5	
Number of cases	369	407	
Greater Cincinnati Survey			
1. 29 May–13 June 91			
Democrat	23.5	22.2	.74, n.s.
Independent	39.0	37.5	
Republican	37.5	40.4	
Number of cases	421	451	
2. 14 Nov.–3 Dec. 91			
Democrat	22.4	27.6	7.51, $p < .05$
Independent	46.0	37.0	
Republican	31.6	35.4	
Number of cases	437	435	
3. 9–29 June 92			
Democrat	21.3	21.3	.19, n.s.
Independent	43.6	44.9	
Republican	35.1	33.8	
Number of cases	436	423	
4. 20 Nov.–14 Dec. 92			
Democrat	25.2	25.7	.97, n.s.
Independent	40.5	42.8	
Republican	34.3	31.4	
Number of cases	504	474	
5. 14 May–2 June 93			
Democrat	29.7	26.1	1.71, n.s.
Independent	36.9	40.1	
Republican	33.4	33.8	
Number of cases	488	459	
Other Surveys			
1. City of Cincinnati, 27 May–3 June 92			
Democrat	30.0	33.3	1.69, n.s.
Independent	46.1	40.0	
Republican	23.9	27.0	
Number of cases	230	215	
2. Cincinnati Area Project[b], 5 Oct.–16 Dec. 91			
Democrat	23.2	27.8	3.4, n.s
Independent	38.1	40.2	
Republican	38.7	32.0	
Number of cases	323	306	

[a] Based on subsample of "likely voters."

[b] Conducted as part of a graduate Practicum in Survey Research at the University of Cincinnati, fall quarter 1991 (see Rademacher 1993).

Responsiveness to Short-Term Forces in the 1992 Campaign

Abramson and Ostrom (1994) have argued, however, that despite this absence of aggregate differences, the Gallup measure is more responsive than the Michigan SRC form to short-term forces, such as changes in presidential approval, particularly during the most recent presidential election campaign. To begin with, they hypothesized that because of George Bush's relatively low approval ratings during the campaign—a situation they believe made the political context much more volatile than normal—the percentage identifying with the Republican party would tend to be higher with the Michigan SRC form (which supposedly taps more of the long-term component of partisanship) than with the Gallup form (which should be more responsive to the short-term decline in Bush's popularity). Support for the Democratic party, they predicted, should be about the same with either form as they believed it should be mostly unaffected by Bush's low approval ratings. Finally, they speculated that because the Gallup form is more responsive to short-term political forces, it might be expected to produce more Independents, especially in a year in which there was a strong independent candidacy in the form of Ross Perot, not to mention other factors leading many voters to rethink their partisanship in a rather volatile political year.

In support of their hypothesis Abramson and Ostrom (1994) reported that more Republicans were found with the Michigan SRC form than with the Gallup form, in all six of their surveys, with the differences varying from 1.1% to 6.6%. The percentage difference in Democratic identifiers, as predicted, was quite small, with the largest difference between forms being 1.3%. Furthermore, in five of the six surveys, the Gallup form produced more Independents, with the differences varying from 2.5% to 6.8%. Though none of these differences were statistically significant (as they acknowledged), they were *all* in the predicted direction.

The relevant question here is, How well did these predictions replicate elsewhere during that same election campaign? Table 1 shows evidence from four State of Ohio polls (March–November 1992); two Greater Cincinnati Surveys, one conducted shortly after the election (1992); and one City of Cincinnati survey (June 1992). Looking first at the Ohio polls, we find that in March 1992, just a week or so after Abramson and Ostrom completed their survey in the state of Michigan, the percentage of Republican identifiers in our Ohio poll was higher with the Gallup form, not with the Michigan form (difference = 1.7%). In both the September and mid-October Ohio polls, the percentage of Republican identifiers was higher with the Michigan form (difference = 1.7% and 6.8%, respectively), as predicted; but in the final preelection Ohio poll (28 October–1 November) of likely voters, the percentage of Republican identifiers was slightly higher with the Gallup form (difference = .3%).

The pattern during the election campaign looks much the same in the Cincinnati surveys. In the June 1992 Greater Cincinnati Survey the percentage of Republican identifiers was higher with the Gallup form, not with the Michigan form (difference = 1.3%). But in the City of Cincinnati survey done just a few weeks before that (27 May–23 June), the percentage of Republican identifiers was higher with the Michigan form (difference = 3.1%), as predicted. Finally, in the postelection Greater Cincinnati Survey (20 November–14 December), at a time when the Democratic triumph of Bill Clinton was still fresh in the minds of voters and consumer confidence, that critical short-term barometer of the public mood, was rising on the tailwinds of the Democratic victory, the percentage of Republican identifiers was higher with the Gallup form (difference = 2.7%), not (as would be predicted) with the Michigan form. And so the story goes: in *four* of the same seven surveys that show no significant *aggregate* differences between the two question forms (just as in the State of Michigan surveys) the percentage of Republican identifiers is higher with the Gallup form, but in the other three surveys it is higher with the Michigan form. The percentage of Democratic and Independent identifiers too, in these same seven surveys, fluctuates back and forth—higher on one form, then higher on the other (see Table 1). These are as hard to distinguish from what we would expect to find by chance as are the insignificant differences we see in 14 of the 15 experiments (Table 1) and in *five* of the six experiments reported by Abramson and Ostrom (1994). The data are clearly telling us that there is not much going on beyond sheer random fluctuation.

Variations in Macropartisanship

The results for the macropartisanship indicator developed by MacKuen, Erikson, and Stimson (1989) tell much the same story. Abramson and Ostrom's (1994) hypothesis predicts that the macropartisanship score (the percentage of major party identifiers who are *Democrats*) should be consistently more Democratic with the Gallup form than with the Michigan form, as the former question should be more responsive to the volatile political context associated with the low approval ratings for President Bush during the 1992 campaign. Indeed, the data from five of their six election campaign surveys shows the ratio of Democrats to Republicans to be higher, as predicted, with the Gallup form. But once again the data from our Ohio studies fails to find this consistent evidence for their hypothesis (see Table 2). In two of the four Ohio polls done during the 1992 election campaign the macropartisanship score was higher on the Michigan form, but in the other two polls it was higher on the Gallup form. In both the June and November–December (postelection) 1992 Greater Cincinnati Surveys, the macropartisanship score was higher on the Michigan form, but in the City of Cincinnati survey in late May and early June 1992, it was slightly higher on the Gallup form. So once more we find a familiar random rocking between the two forms of the questions—now somewhat higher with the Gallup form, then somewhat higher with the Michigan form.

Table 2 also shows some variation over time in the macropartisanship scores observed in our eight Ohio polls, alongside the trend in presidential approval from the same polls. As Abramson and Ostrom (1994) have carefully pointed out, our April 1991 Ohio poll, which was conducted at a time when presidential approval was near an all-time high shortly after the Persian Gulf War and when Republicanism was surging in the national Gallup polls, the macropartisanship score (percent Democrat) was, as would be predicted, noticeably higher on the Michigan form (thus showing an *aggregate* difference between forms in response to a major short-term force). In October 1991, presidential approval, as measured in both the Ohio and national polls, had dropped considerably (down 14.6% in Ohio) and, as Abramson and Ostrom would predict, the percentage of Democratic identifiers is now higher on the more responsive Gallup form and moves down correspondingly on the Michigan form. By March 1992, presidential approval had plummeted even further in the Ohio poll (down 17.4%), as well as in the national polls. In fact, by that time, it was well below 50% (43.7%), making the political context, as Abramson and Ostrom would argue, quite volatile. But the percentage of Democratic identifiers does not rise further on the more

TABLE 2

Macropartisanship Scores by Question Form in Fifteen Experiments

SURVEY	GALLUP	MICHIGAN	% APPROVING PRESIDENT IN OHIO POLL
Ohio Poll			
1. 24–30 Apr. 91	49.3	57.2	75.7
2. 2–11 Oct. 91	55.2	50.7	61.1
3. 20–30 Mar. 92	48.8	49.4	43.7
4. 9–19 Sept. 92	54.1	50.9	39.2
5. 16–25 Oct. 92	59.2	51.5	Not Asked
6. 28 Oct.–1 Nov. 92[a]	51.0	51.9	Not Asked
7. 3–10 Feb. 93	55.5	57.2	42.4
8. 3–14 May 93	48.6	50.4	38.1
Greater Cincinnati Survey			
1. 29 May–13 June 91	38.5	35.5	—
2. 14 Nov.–3 Dec. 91	41.5	43.8	—
3. 9–29 June 92	37.8	38.7	—
4. 20 Nov.–14 Dec. 92	42.4	45.0	—
5. 14 May–2 June 93	47.1	43.6	—
Other Surveys			
1. City of Cincinnati, 27 May–3 June 92	55.7	55.0	—
2. Cincinnati Area Project, 5 Oct.–16 Dec. 91	37.5	46.5	—

Note: Scores are percentages of major party identifiers who are Democrats.

[a] Likely voters.

responsive Gallup form, as would be predicted. It actually drops back to a level below that observed in the April 1991 Ohio poll, which Abramson and Ostrom (1994) have cited as evidence supporting their predictions on macropartisanship. Notice, too, that in the March 1992 Ohio poll, the percentage of Democratic identifiers on the Michigan form barely moves from the base line of October 1991 (50.7 to 49.4), whereas the plunging presidential approval rating would lead us to expect a much sharper drop and a corresponding rise in Republican identifiers on the Michigan form of the question, which supposedly better measures the long-term component of partisanship. In the September 1992 Ohio poll, the macropartisanship score shoots back up 5.3% on the Gallup form, but barely moves on the Michigan form, while presidential approval nudges down just a bit more (43.7 to 39.2). In the mid-October poll it shoots up again on the Gallup form (5.1%) and moves just a little on the Michigan form. And in the final preelection poll of likely voters there is little or no difference between forms in the macropartisanship scores, though it is slightly higher on the Michigan form.

Notice, finally, that the macropartisanship score, as Abramson and Ostrom would predict, rises on the Gallup form from 37.8% in the June 1992 Greater Cincinnati Survey to 42.4% in the postelection Greater Cincinnati Survey of November–December. But unexpectedly, it rises even more on the Michigan form in these same two surveys (38.7% to 45.0%). Once again, it all looks rather random and unpredictable and the hypothesis about the greater responsiveness of the Gallup form appears rather unsalvageable without a lot of ad hoc, unfalsifiable interpretations after each and every point in time. Before we are tempted to do that, let us take a look at the correlational evidence on which of the two forms is more responsive to short-term evaluations and electoral preferences.

Correlations with Short-Term Political Evaluations and Electoral Preferences

Abramson's and Ostrom's (1994) thesis clearly predicts that responses to the Gallup form should be more highly correlated with short-term economic, political, and electoral evaluations, such as presidential approval, consumer confidence, candidate evaluations, voting preferences, and the like than should responses to the Michigan form. The evidence from their analysis appears to support their prediction. In 30 out of 33 comparisons, they found that the responses to the Gallup form were more highly correlated with various short-term indicators than were those given to one of their two recoded versions of the Michigan form (SRC 1). With the other recoded version of the Michigan form (SRC 2), their results were even better: all 33 comparisons were in the predicted direction (but only *three* statistically significant), leading them to conclude that "the Gallup question is more likely to capture short-term preferences than the Michigan SRC question." It all looks rather impressive; but once more, we must ask, How well does it replicate?

The answer given in Table 3 is, *not very well.* Consider first the correlations from the April 1991 Ohio poll.[6] This same Ohio poll, as Abramson and Ostrom (1994) have pointed out, shows the surge in Republican identifiers on the Gallup form corresponding to the soaring presidential approval ratings for George Bush shortly after the Persian Gulf War; and the macropartisanship score (percent of major party identifiers who are *Democrats*) is higher on the Michigan form, as they would predict. But as this Republican presidential tide is rising, we find that the Michigan form of the party identification question, not the supposedly more responsive Gallup form, is somewhat more sensitive to such shifts in presidential fortunes. In each case, we find that the correlation between party identification and presidential approval ratings is higher with the Michigan form than with the Gallup form, not only for the overall approval rating but also for approval of the president's performance in handling the economy, foreign affairs, and the very recent, *short-term event* of the Persian Gulf War.

The same pattern shows up on the gubernatorial and senatorial approval and evaluation ratings, as well as in the senatorial trial heat votes: the correlations are generally higher with the Michigan form. But on the consumer confidence measures, with one exception, the correlations tended to be higher with the Gallup form. And for the most part, this general pattern repeats itself in the October 1991 Ohio poll (see Table 3).

Consider next the March 1992 Ohio poll conducted, again, just a week or so after Abramson's and Ostrom's Michigan poll. We find that the correlation between party identification and overall presidential approval is a bit higher on the Michigan form, not on the supposedly more responsive Gallup form. Similarly, the ratings for the president's handling of the economy and foreign affairs more highly correlated with the Michigan form of the party identification question, an additional piece of evidence not available in Abramson's and Ostrom's analysis. Consumer confidence ratings (another key short-term indicator in this debate) also tended to be somewhat more highly correlated with responses with the Michigan form of the question, not with the Gallup form. Gubernatorial and lieutenant-gubernatorial approval ratings were somewhat higher with the Gallup form. But then when we look at the results for short-term favorability ratings of different presidential candidates at that time and voting preferences in various presidential trial heats, the correlations are noticeably higher with the Michigan form.

We could go on multiplying examples and counterexamples before the campaign, during the campaign, and after the campaign. But it would be best here to summarize what is shown in Table 3. Of the 134 comparisons in Table 3, involving a great variety of short-term evaluations and electoral preferences at various times and under various economic and polit-

TABLE 3

Product–Moment Correlations between Partisanship and Short-Term Political and Economic Evaluations by Question Form

SURVEY	MICHIGAN	GALLUP
Ohio Poll		
1. 24–30 Apr. 91		
Pres. approval		
General	**.30** (337)	.25 (312)
Economy	**.36** (337)	.31 (313)
Foreign affairs	**.31** (338)	.21 (312)
Persian Gulf	**.25** (338)	.19 (313)
Gubernatorial approval	**.27** (334)	.20 (311)
U.S. senatorial approval	**.19** (338)	.13 (313)
U.S. senatorial evaluation	**.12** (310)	.02 (280)
U.S. Senate pref.		
DeWine/Glenn	**.54** (195)	.53 (198)
Douglas/Glenn	**.56** (186)	.38 (192)
McEwen/Glenn	**.54** (194)	.49 (197)
Petro/Glenn	**.46** (187)	.43 (185)
Taft/Glenn	.51 (206)	**.54** (196)
Consumer confidence		
Personal financial outlook	.12 (336)	**.22** (308)
Outlook U.S. business	**.19** (333)	.15 (303)
Outlook for major purchases	.00 (329)	**.15** (299)
Overall index	.17 (325)	**.21** (287)
2. 2–11 Oct. 91		
Pres. approval		
General	**.39** (317)	.34 (296)
Economy	**.34** (317)	.28 (295)
Foreign affairs	.21 (317)	**.29** (296)
Gubernatorial Approval	**.26** (317)	.24 (296)
U.S. senatorial approval	**.24** (317)	.23 (296)
U.S. senatorial evaluation	**.23** (268)	.20 (266)
U.S. Senate pref.		
DeWine/Glenn	**.60** (186)	.55 (178)
Douglas/Glenn	**.57** (175)	.50 (169)
McEwen/Glenn	**.53** (184)	.50 (181)
Petro/Glenn	.49 (177)	**.50** (169)
Taft/Glenn	**.51** (180)	.48 (168)
Consumer confidence		
Personal financial outlook	.05 (309)	**.15** (292)
Outlook U.S. business	.14 (311)	**.20** (290)
Outlook for major purchases	.10 (304)	**.15** (282)
Overall index	.15 (293)	**.26** (275)
3. 20–30 Mar. 92		
Pres. Approval		
General	**.49** (394)	.47 (387)
Economy	**.45** (394)	.37 (387)
Foreign affairs	**.32** (394)	.30 (387)
Gubernatorial approval	.21 (393)	**.25** (387)
Lt. gov. approval	.18 (393)	**.20** (385)
U.S. senatorial approval	**.21** (393)	.20 (387)
U.S. senatorial evaluation	.21 (357)	.21 (358)
Favorability rating		
Jerry Brown	**.16** (119)	.15 (119)
George Bush	**.52** (352)	.46 (337)
Bill Clinton	**.46** (204)	.33 (196)
Michael DeWine	.33 (62)	**.52** (68)
John Glenn	.19 (309)	**.23** (303)
Pres. vote pref., Bush/Clinton	**.67** (246)	.57 (233)
Pres. vote pref., Bush/Brown	**.56** (248)	.44 (237)
U.S. Senate vote pref.	.38 (257)	**.41** (250)

TABLE 3 (continued)

SURVEY	MICHIGAN	GALLUP
Consumer Confidence		
Personal financial outlook	**.20** (382)	.09 (377)
Outlook U.S. business	.30 (379)	.30 (377)
Outlook for major purchases	**.19** (387)	.17 (374)
Overall index	**.33** (366)	.31 (356)
4. 9–19 Sept. 92		
Presidential approval, general	.56 (478)	**.60** (411)
Gubernatorial approval	**.23** (480)	.20 (412)
Lt. gov. approval	.22 (479)	**.23** (412)
U.S. senatorial approval	**.31** (479)	.30 (412)
Favorability rating		
George Bush	.59 (453)	**.65** (374)
Bill Clinton	.59 (357)	**.64** (303)
Michael DeWine	**.36** (109)	.31 (89)
John Glenn	**.35**.(352)	.34 (312)
Albert Gore	.44 (237)	**.49** (212)
Dan Quayle	**.50** (372)	.49 (314)
Pres. Vote Pref.		
Bush/Clinton	.78 (303)	**.79** (270)
Bush/Clinton & Perot	.76 (309)	.76 (286)
Bush & Pert/Clinton	.77 (309)	.77 (286)
U.S. Senate Vote Pref.	**.55** (327)	.54 (278)
Consumer confidence		
Personal financial outlook	**.14** (467)	.09 (401)
Outlook U.S. business	**.19** (455)	.17 (394)
Outlook for major purchases	.17 (456)	**.18** (394)
Overall index	.21 (427)	**.22** (373)
5. 16–25 Oct. 92		
Favorability Rating		
George Bush	**.61** (311)	.50 (320)
Bill Clinton	**.67 (283)**	.61 (283)
H. Ross Perot	**.17** (216)	.04 (224)
Michael DeWine	**.44** (150)	.33 (134)
John Glenn	**.42** (259)	.35 (283)
Comfort Rating		
George Bush	**.62** (330)	.54 (343)
Bill Clinton	**.66** (326)	.53 (342)
H. Ross Perot	**.07** (323)	.03 (341)
Pres. vote pref.		
Bush/Clinton	**.77** (239)	.69 (235)
Bush/Clinton & Perot	**.65** (292)	.59 (288)
Bush & Perot/Clinton	**.70** (292)	.61 (288)
U.S. Senate vote pref.	**.54** (200)	.41 (300)
Best cand. in debates		
Bush/Clinton	**.47** (184)	.42 (221)
Bush/Clinton & Perot	.29 (309)	.31 (328)
Bush & jPerot/Clinton	**.41** (309)	.33 (328)
Next best in debates		
Bush/Clinton	**.10** (187)	.02 (200)
Bush/Clinton & Perot	**.18** (296)	.07 (317)
Bush & Perot/Clinton	.06 (296)	**.09** (317)
Consumer confidence		
Personal financial outlook	.06 (407)	.06 (417)
Outlook U.S. business	**.02** (381)	.00 (393)
Outlook for major purchases	.06 (402)	**.10** (417)
Overall index	**.05** (368)	.03(372)
6. 28 Oct.–1 Nov. 92		
Favorability rating		
George Bush	.58 (528)	**.62** (524)
Bill Clinton	.61 (524)	**.65** (523)
H. Ross Perot	**.06** (520)	.03 (508)
Michael DeWine	.37 (473)	**.41** (480)
John Glenn	.40 (518)	**.45** (512)

TABLE 3 (continued)

SURVEY	MICHIGAN	GALLUP
Comfort rating		
George Bush	.57 (533)	**.64** (524)
Bill Clinton	.62 (532)	**.69** (521)
H. Ross Perot	**.10** (528)	.05 (519)
Pres. vote pref.		
Bush/Clinton	.72 (386)	**.77** (410)
Bush/Clinton & Perot	.57 (477)	**.69** (480)
Bush & Perot/Clinton	.66 (477)	**.69** (480)
U.S. Senate vote pref.	.50 (481)	**.58** (469)
7. 3–10 Feb. 93		
Presidential approval, general	.37 (398)	**.41** (407)
Approval		
Hillary appointment	.22 (398)	**.34** (408)
Gays-in-military plan	**.33** (397)	.27 (406)
Abortion policy decision	**.17** (397)	.11 (404)
Gubernatorial approval, general	.19 (398)	**.21** (407)
Gubernatorial approval, state budget	.16 (396)	**.18** (407)
Consumer Confidence		
Personal financial outlook	.07 (388)	**.14** (401)
Outlook U.S. business	.11 (386)	**.11** (391)
Outlook for major purchases	.01 (380)	.01 (390)
Overall index	.03 (364)	**.10** (373)
8. 3–14 May 93		
Presidential approval, general	**.62** (407)	.54 (369)
Approval, Hillary Clinton	**.40** (406)	.33 (369)
Gubernatorial approval, general	.18 (406)	**.19** (368)
Consumer confidence		
Personal financial outlook	.17 (404)	**.18** (366)
Outlook U.S. business	.12 (388)	**.31** (354)
Outlook for major purchases	**.07** (389)	.01 (348)
Overall index	.13 (370)	**.27** (336)
Favorability rating		
Richard Celeste	.26 (166)	**.33** (155)
Michael DeWine	.40 (82)	**.52** (85)
Joel Hyatt	**.46** (70)	.24 (53)
George Voinovich	.22 (211)	**.25** (172)
U.S. Senate vote pref., Celeste/DeWine	**.62** (271)	.61 (244)
U.S. Senate vote pref., Hyatt/DeWine	.66 (261)	**.67** (232)
Gubernatorial vote pref., Celeste/Voinovich	**.55** (292)	.54 (269)
Greater Cincinnati Survey 2–29 June 92		
Pres. vote pref.		
Clinton/Bush	.70 (196)	**.72** (196)
Clinton/Bush & Perot	**.56** (284)	.54 (291)
Clinton & Perot/Bush	.51 (284)	**.53** (291)
20 Nov.–14 Dec. 92		
Pres. vote		
Bush/Clinton	.79 (304)	**.80** (330)
Bush/Clinton & Perot	.69 (347)	**.72** (370)
Bush & Perot/Clinton	.74 (347)	**.75** (370)
3–14 May 93, Pres. approval, general	.57 (458)	.57 (488)
Cincinnati Area Project, 5 Oct.–16 Dec. 91		
Pres. approval, General	.49 (303)	**.50** (329)

Note: The higher correlation in each comparison has been bolded for emphasis. All coefficients are Pearson r's, with the variables coded so that the predicted zero-order relationships are positive.

ical conditions, we found that 70 of the correlations (52.2%) were higher when the Michigan form was used, and 57 (42.5%), when the Gallup form was used, with 7 tied cases (5.2%). And only 7 of these 134 comparisons produced a statistically significant difference, 4 or them being significantly higher on the Michigan form, 3 on the Gallup form—just about what would be expected by chance (7/134 = .052).[7]

If we restrict our analysis to the surveys done during the campaign, the pattern is nearly identical. Of the 78 available comparisons, 41 (52.6%) show that the correlations were higher with the Michigan form, and 31 (39.7%), with the Gallup form, with 6 tied cases (7.7%).

If we confine the comparisons to those involving presidential approval ratings, we find there are 15 possible comparisons, 10 of which (66.7%) produce higher correlations with the Michigan form, and 4 (26.7%), with the Gallup form, with 1 tie (6.7%). Short-term "favorability" and "comfort" ratings of the presidential candidates during the campaign behave much the same way. Of 17 comparisons, 11 (64.7%) were higher with the Michigan form, 6 (35.3%), with the Gallup form. Presidential, senatorial, and gubernatorial trial heats tend in the same direction: the correlations are generally higher on the Michigan form (see Table 3). In contrast, the consumer confidence ratings were only weakly related to partisanship in most of the comparisons shown in Table 3 regardless of the question form that was used, though the correlations tended to be slightly higher with the Gallup form in a majority of the cases.

To summarize, the evidence from this correlational analysis of short-term evaluations and electoral preferences is mixed at best. If anything, the data suggest that the Michigan form of the question may be the better predictor of the two under a variety of political, economic, and electoral conditions—in this case, over a two-year period. And to the extent that such predictive validity implies higher reliability of measurement (see Green and Schickler, 1993), a rival hypothesis about greater *random* error with the Gallup form, as opposed to greater responsiveness in the sense of true variance, becomes more plausible as an explanation of at least some of the findings in this controversy.

Responsiveness to Question Order and Context Effects

We have one more piece of empirical evidence on the relative responsiveness of the two question forms. In the Greater Cincinnati Survey conducted shortly after the 1992 presidential election (20 November—December 14), we varied not only the wording of the party identification question but also the order and context in which it was asked. In one split-half of the sample it was asked just *before* a question about how respondents said they had voted in the presidential election (Bush, Clinton, or Perot); and in the other split-half, it was asked just *after* the question on how they had voted.[8]

Now we already know that the wording of this question did not make a significant difference in the marginal distribution of responses to the two forms, as reconfirmed by the chi-squared value shown in Table 4 (X^2 = .97, df = 2, n.s.). But the order and context in which it was asked did make a significant difference in the aggregate results (X^2 = 8.47, df = 2, $p < .02$). When the party identification question was asked *after* the vote question, the percentage of Independent and Democratic identifiers tended to be significantly higher than when it was asked *before* the vote question, suggesting that it was indeed responsive to the recent election results. Now if the Gallup form is more responsive to short-term stimulation, then we would expect that it would be more sensitive than the Michigan form to this *immediate* (less than 30 seconds), contextual effect of making highly salient to the respondent, when answering the party identification question, how he or she had voted in the recent presidential election.

The analysis in Table 4, however, tells us that the order effect occurs with both the Gallup and the Michigan forms of the question, so that the three-way interaction is not statistically significant. Moreover, if there is any trend in these data, it is that the order effect was somewhat larger with the Michigan form: the difference in the percentage of Democratic and Independent identifiers was noticeably larger with the Michigan form than with the Gallup form. So here we have a clear experimental manipulation of the key theoretical variable of "short-term stimulation" in a postelection survey laboratory, and the evidence on the supposedly greater responsiveness of the Gallup form is not only negative, but it tends, much like the correlational evidence in Table 3, to run in the opposite direction, making the Michigan form look somewhat more responsive and, to that extent, perhaps more reliable and valid as well.

CONCLUSIONS

The weight of the evidence from the multiple experiments and replications we have carried out, plus that reported by Abramson and Ostrom (1994), as well as by Green and Schickler (1993), leads first of all, to one very clear conclusion: at any given point in time the distribution of political party identification responses will generally be the same, regardless of whether one uses the Gallup or Michigan form of the question. Furthermore, the data from our surveys conducted during the 1992 election campaign clearly do not support Abramson's and Ostrom's conclusions about the greater responsiveness of the Gallup form to the short-term forces operating during the campaign. For one thing, we did not find consistently more Republicans and Independents with the Gallup form or consistently more Democrats with the macropartisanship indicator. For another (and more importantly), we did not find that responses to the Gallup form

TABLE 4

Political Party Identification by Question Form, by Question Order and Context, in the 1992 November–December Greater Cincinnati Survey (%)

PARTY IDENTIFICATION	ORDER			
	REPORTED 92 VOTE/PARTY ID[a]		PARTY ID/REPORTED 92 VOTE[b]	
	GALLUP	MICHIGAN	GALLUP	MICHIGAN
Democrat	24.0	20.6	26.6	31.4
Independent	43.3	48.0	37.3	37.2
Republican	32.7	31.5	36.1	31.4
Total	100.0	100.1	100.0	100.0
N	263	248	241	226

[a] X^2 = 1.32, df = 2, n.s.

[b] X^2 = 1.71, df = 2, n.s.

Note: Party ID by wording: X^2 = .97, df = 2, n.s.; party ID by order; X^2 = 8.47, df = 2, $p < .02$; three-way interaction (party ID by wording by order): X^2 = 2.06, df = 2, n.s.

were more consistently related to short-term evaluations and electoral preferences than the Michigan form during the campaign, before the campaign, or after the campaign. If anything, we found that responses to the Michigan form, not the Gallup form, were more responsive to such short-term forces, suggesting that the former may be the more reliable and predictively valid of the two. If the Gallup form does generate a slightly larger random error component (because respondents interpret the expression "in politics, as of today" in a random or quasi-random manner), that, and not systematic responsiveness would help explain some of the apparently inexplicable differences in variability between the two forms observed by Abramson and Ostrom. Random error, rather than true change, thus becomes a somewhat more plausible rival hypothesis.[9]

Finally, the convergent evidence from the analysis of all these experiments and replications makes it very difficult not to agree with MacKuen's, Erickson's, and Stimson's conclusion that "the underlying phenomenon of macropartisanship does not depend on any particular question wording for its substance" (1992, 479). (For a similar conclusion based on alternative measurements of party identification, see Franklin 1992). Given that the survey is conducted by the same organization, using the same mode of interviewing, the Gallup and Michigan forms of the party identification question can, for all practical purposes, be regarded as functionally equivalent, interchangeable indices.[10] Those who disagree with this conclusion bear a heavy burden of disproof; for, rumors to the contrary and analogous precedents (Bishop, Oldendick, and Tuchfarber 1978; Sullivan, Pierson, and Marcus 1978) notwithstanding, changes in question wording do not always make a significant difference in one's results and conclusions. Artifacts, as Schuman (1982) would put it, are often in the mind of the beholder.

APPENDIX: INTERVIEWING PROCEDURES, SAMPLING AND RESPONSE RATES

Interviewing Procedures

The data used in this paper come from eight statewide Ohio polls, five Greater Cincinnati Surveys (of Hamilton County, Ohio), a City of Cincinnati Survey, and a Greater Cincinnati Area Project. The Ohio polls, the Greater Cincinnati Surveys, and the City of Cincinnati Survey were conducted by the University of Cincinnati Institute for Policy Research (IPR) at the institute's centrally supervised Computer-assisted Telephone Interview (CATI) facility by interviewers trained at the institute. (The Cincinnati Area Project was a "paper-and-pencil" telephone survey; CATI was not used.) Most of the interviewers have been with the institute for several years (many for 10 or more), although a few students were used. Interviewers were not informed of the purpose of any of the experiments and have not seen any of the results of this research.

Sampling

Households were selected using random-digit-dialing procedures. First, with the aid of the computer, one of the three-digit telephone exchanges currently used in the area (e.g., 772) is randomly selected. The computer then randomly selects one of the "working blocks" (the next pair of digits, e.g., 64) and attaches it to the randomly selected exchange. Finally, the computer program then generates a two-digit random number between 00 and 99 (e.g., 57) and attaches it to the previously selected prefix (772) and working block (64), resulting in a complete telephone number (e.g., 772-6457). This procedure is then repeated numerous times by the computer to generate more random numbers, until we have a sufficient quantity to conduct the survey. The end result is that each household in the area with a telephone, has an equally likely chance of being selected into the sample. The samples used in all 15 surveys were purchased from Survey Sampling, Incorporated, Fairfield, Connecticut.

In all 15 surveys, respondents were selected within households using the last-birthday method. If the number contacted is a residential number, the interviewer then randomly selects a member of the household by asking to speak with the person currently living in the household who is age 18 or older and who has had the most recent birthday. This selection process ensures that every adult (18 years of age or older) in the household has an equally likely chance of being included in the survey. No substitutions are allowed. If, for example, the randomly selected adult is not at home when the household is first contacted, the interviewer cannot substitute by selecting someone else who just happens to be there at the time. Instead, he or she must make an appointment to call back when the *randomly selected person* is at home. In this way, respondent selection bias is minimized. A selected respondent who refuses to participate in the study is recontacted by a senior interviewer, who attempts to complete the interview. This procedure, though costly, significantly reduces the percentage of respondents who refuse to complete a survey, thereby increasing completion rates.

Split-Half Assignment

In the eight Ohio polls, the five Greater Cincinnati Surveys, and the City of Cincinnati Survey, respondents were randomly assigned by the IPR CATI system to receive either the SRC form of the party identification ("Generally speaking") or the Gallup form ("In politics, as of today"). In the Cincinnati Area Project, respondents were randomly assigned to receive one of the two questionnaire forms.

Weighting

The data reported herein were all unweighted. An analysis of the same surveys using weighted data based on demographic characteristics of the respondents (age, education, race, and sex) produced results not differing significantly from those we report.

Completion Rates

Completion rates are shown in Table A-1.

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TABLE A-1

Completion Rates

SURVEY	HOUSEHOLDS CONTACTED	INTERVIEWS COMPLETED	% COMPLETED	FAILURE TO INTERVIEW		
				% REFUSALS	% INCOMPETENT[a]	% UNAVAILABLE[b]
Ohio Poll						
1. 24–30 Apr. 91	991	684	69.0	13.9	6.2	10.9
2. 2–11 Oct. 91	n.a	646	—	—	—	—
3. 20–30 Mar. 92	1,273	816	64.1	10.7	5.6	19.6
4. 9–19 Sept. 92	1,468	976	66.5	16.1	5.9	11.5
5. 16–25 Oct. 92	1,450	940	64.8	11.2	5.3	18.6
6. 28 Oct.–1 Nov. 92	1,411	931[c]	66.0	7.3	3.2	23.5
7. 3–10 Feb. 93	1,276	854	66.9	13.4	4.9	14.7
8. 3–14 May 93	1,257	816	64.9	17.7	4.5	13.0
Greater Cincinnati Survey[d]						
1. 29 May–14 June 91	1,538	1,028	66.8	13.4	10.7	9.0
2. 14 Nov.–3 Dec. 91	1,328	959	72.2	15.2	5.5	7.1
3. 9–29 June 92	1,361	954	70.1	13.1	5.2	11.6
4. 20 Nov.–14 Dec. 92	1,647	1,105	67.1	14.0	4.6	14.3
5. 20 May–2 June 93	1,556	1,055	67.8	15.2	4.6	12.5
City of Cincinnati, 27 May–3 June 93	719	482	67.0	9.0	7.0	17.0
Cincinnati Area Project,[e] 5 Oct.–16 Dec. 92	1,085	691	63.7	—	—	—

[a] Due to language barrier, hearing problem, senility, illness, or illiteracy.

[b] Due to business, vacation, or other cause.

[c] Identified as "likely voters" through a combination of characteristics. All were registered voters, and all said they planned to vote on 3 November. Potential turnout levels were estimated from data provided by the Ohio Secretary of State, and the most likely voters were identified based on whether they voted in the 1988 general election and their stated level of interest in this particular election.

[d] Hamilton County, Ohio.

[e] This was part of a Practicum in Survey Research conducted by graduate students in the political science department. None of the students involved was aware of the purpose of the experiment.

RESPONSE

For the past three decades political scientists have analyzed the changing distribution of partisan loyalties among the U.S. electorate. MacKuen, Erikson, and Stimson's (1989) study of aggregate levels of Gallup partisanship between 1945 and 1987 is especially important, because their time series analyses found substantial short-term variation in partisan loyalties, variation driven by short-term assessments of economic conditions. Their findings challenged the traditional view of party identification advanced by Campbell and his colleagues (1960) and led MacKuen, Erikson, and Stimson to argue that political scientists should shift their attention from long-term patterns of partisan change to short-term changes in the aggregate distribution of partisan loyalties.

MacKuen, Erikson, and Stimson's basic measure was the percentage of partisans who said they were Democrats, which they labeled *macropartisanship*. We were skeptical of these conclusions because they were based upon analyses of a question that seemed to tap short-term partisan sentiments, rather than long-term partisan loyalties. We argued that at least part of the variation in Gallup macropartisanship resulted from the short-term focus of the Gallup question itself (Abramson and Ostrom 1991).

Although aggregate-level analyses found that responses to the Gallup question were more responsive to short-term change than responses to the Michigan SRC question, these analyses could not definitely identify the cause of these differences. We recommended that political scientists conduct question-wording experiments that might shed light on why the Gallup and Michigan SRC results differed from one another (MacKuen et al. 1992, resp.).

Bishop, Tuchfarber, and Smith have conducted an extensive series of question-wording experiments. They present results from 15 telephone polls conducted in Ohio between April 1991 and June 1993 and compare results from the Gallup and Michigan SRC questions. They find few differences and conclude, "Given that the survey is conducted by the same organization, using the same mode of interviewing, the Gallup and Michigan forms of the party identification question can, for all practical purposes, be regarded as functionally equivalent, interchangeable indices" (953).

Except for a handful of experimental surveys, the Gallup and the Michigan SRC measures are *not* used by the same organizations, and since 1988, Gallup has relied mainly upon telephone polls. Therefore, even if the Gallup and SRC measures are functionally equivalent when used by the same survey organization, the actual Gallup data might have limited utility for making inferences about the Michigan SRC party identification measure. In fact, Bishop, Tuchfarber, and Smith's own discussion raises an important additional question about the comparability of the Gallup time series and results based upon the U.S. National Election Studies (NES). They point out that in actual practice the Gallup organization has asked its partisanship question in a wide variety of contexts and that these differing contexts may contribute to greater variability in the Gallup time series.

Despite their arguments, the weight of the evidence suggests that the Gallup item itself is more responsive to short-term political forces than the SRC measure. Bishop and his colleagues challenge the question-wording experiments we conducted in six statewide surveys of Michigan between 8 March and 26 October 1992 (Abramson and Ostrom 1994). In fact, their findings differ from ours in only one minor respect. They find no systematic tendency for responses to the Gallup question to be more strongly related to short-term indicators (e.g., presidential approval, vote preferences, and consumer confidence) than responses to the Michigan SRC questions. We found that in 30 of 33 comparisons, the responses to the Gallup question were more strongly related to these indicators than responses to SRC_1 (the Michigan measure excluding independents), although, as we reported, only three of these differences were significant.

In other respects, our findings are either similar to those presented by Bishop, Tuchfarber, and Smith's findings or are based upon different types of experiments. For example, Bishop and his colleagues compare aggregate-level differences between alternative question formats in their surveys and ours, and we both find relatively small differences. We agree that during many periods there will be little difference between aggregate levels of macropartisanship, whether the Gallup or the SRC measure is used. But during periods of political volatility the Gallup preface will accentuate differences and the Michigan preface will attenuate them. The strongest evidence comes from the divergence between surveys using the Gallup and SRC measures conducted shortly after the Persian Gulf War, when surveys using the Gallup measure showed a surge in Republicanism but surveys using the SRC measure did not. (For a summary, see MacKuen et al. 1992, resp.). Bishop, Tuchfarber, and Smith's statewide Ohio survey conducted in April 1991 supports findings reached by national surveys and provides important new evidence, because differences between the two measures cannot result from house effects—that is to say, from differing procedures employed by different survey organizations. Fifty-one percent of the major party supporters were Republicans when the Gallup question was employed, but only 43% when the SRC measure was used (see Table 2).

The period when our surveys were conducted was not as dramatic, but it was clearly a politically volatile time with negative sentiments toward George Bush among the electorate. We therefore predicted that Gallup macropartisanship would be more Democratic than SRC macropartisanship. This was true in five of our six surveys, and the mean difference across all six surveys shows the Michigan electorate to be 3.2 points more Democratic when the Gallup measure is used. Bishop and his colleagues present three polls of the Ohio electorate conducted during this time period. In two of them Gallup macropartisanship is more Democratic than SRC macropartisanship, and in one the Ohio electorate is slightly less Democratic (see Table 2). But the mean difference for their three statewide surveys shows the Ohio electorate to be 3.4 points more Democratic when the Gallup measure is employed, a result remarkably similar to ours. Because small differences in macropartisanship scores can affect time-series estimates (Abramson and Ostrom 1991), the tendency for the Gallup measure to accentuate short-term change raises questions about using the Gallup time series to make inferences about the Michigan SRC measure.

Other findings we present are based upon different types of experiments from those Bishop and his colleagues conducted and are not discussed by them. For example, we present the results of two cross-sectional surveys that compared responses among the same respondents who were asked SRC and Gallup questions about 12 to 15 minutes apart. About 1 respondent in 8 was classified differently. Granted, these differences could result from contrast effects, in which responding to the first partisanship question leads respondents to respond differently to the second. But our findings are remarkably consistent with those based on a national survey conducted in December 1973 by the National Opinion Research Center (NORC) in which differing partisanship questions were scattered over lengthy in-person interviews.[11]

More importantly, our experiment included a four-wave panel study.[12] Our panel allowed us to compare respondents who were consistently asked the Michigan SRC questions with those consistently asked the Gallup question. Eleven over-time comparisons were possible and in all 11 comparisons, the responses to the SRC measure were more stable than the responses to the Gallup measure. In 6 of these 11 comparisons, these differences were statistically significant at the .05 level. These results strongly suggest that responses to the Gallup question are more subject to short-term forces than responses to the Michigan SRC question, although we recognize that an ideal design would have included additional control groups (e.g., a control group interviewed on only the first and fourth wave of our panel).

If most of our findings are either similar or unrelated to those presented by Bishop and his colleagues, why is there a controversy between us? The important issue is not why our results and theirs differ in the relative magnitude of correlation coefficients that are seldom significantly different from each other, regardless of whether the Gallup or SRC measure is employed. Rather, it is whether the Gallup time series can be used to reach conclusions about research that employs the Michigan SRC measure. A careful reading of their argument raises additional doubts about the utility of the Gallup times series. Bishop and his colleagues argue that over-time variation in the Gallup measure may result from the "more rapid turnover of topics, shifts in response to preceding questions about the most important problem(s) facing the country, presidential approval, candidate trial heats, and the like, as compared to the more slowly evolving, less variable contexts of the NES and the NORC GSS" (n. 9). The "variable context hypothesis" is important and should be tested. If contextual differences prove to be important, MacKuen, Erikson, and Stimson's (1989) findings would be called into question. Their analysis was not based upon all of the available Gallup surveys (in which the varying effects of contextual differences might even out) but upon the first Gallup survey of every odd-numbered month, aggregated into quarters.[13]

Despite minor discrepancies between Bishop, Tuchfarber, and Smith's findings and our own, differences in question wording remain the most likely explanation for the greater aggregate-level variation in Gallup macropartisanship. Of course, the Gallup measure and the Michigan measure usually yield similar results, and cross-sectional surveys that employ the Gallup measure will usually yield similar results to those using the basic SRC party identification question. But political scientists should be cautious about using time-series analyses of the postwar Gallup data to reach conclusions about either the Michigan SRC party identification measure itself or the best way to study changing party identification among the U.S. electorate.

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Notes

The first part of this Controversy is the revised version of a paper presented at the 1993 meeting of the American Political Science Association, Washington; for their constructive comments, criticisms, and suggestions, Bishop, Tuchfarber, and Smith would like to thank Paul Abramson, Charles Franklin, Richard Niemi, Michael MacKuen, and James Stimson. Abramson and Ostrom are grateful to Ada W. Finifter, Jim Granato, and Brian D. Silver for their suggestions.

1. For a brief, recent overview of the meaningfulness and measurement of this central concept, see Niemi and Weisberg 1993a, 210–23. See also their review of the various controversies in the measurement of party identification (Niemi and Weisberg 1993b, 268–83). See, too, the recent special issue on party identification in *Political Behavior* (Franklin, Brody, and Sniderman 1992).

2. See Belknap and Campbell 1951/52, in which the Michigan SRC form of the question first developed, and Campbell et al. 1960, in which it evolved into the central, intervening variable of the "Michigan model" of American voting behavior. For a brief historical review of the measurement of *party allegiance*, *party affiliation*, and *party identification* as these concepts emerged in public opinion and electoral research in the United States, see Rademacher 1993.

3. See, esp. Krosnick and Berent 1993; Weisberg 1980.

4. See the Appendix for documentation of the sampling procedures and response rates in these surveys and see Abramson and Ostrom 1994, app., for a comparison of the quality of these two data collections.

5. We want to thank Paul Abramson and Charles Ostrom for providing us with copies of various drafts of their manuscript, as well as copies of the questionnaires they have used

in their surveys. It illustrates well the spirit of scientific exchange between rival investigators with competing hypotheses.

6. The correlations presented in Table 3 are based on a fully unfolded, seven-point party identification scale for both the Michigan SRC and Gallup forms of the question, which maximizes the measurement of variance in the responsiveness of these two forms to the various short-term indicators shown in this table. We have also done the same analysis of correlations, using Abramson's and Ostrom's (1993) dichotomous recoding of the Michigan SRC and Gallup forms, which either excludes "leaning independents" (SRC1 and GAL1) or includes them (SRC2 and GAL2), with "true independents" deleted completely from the analysis. Our analysis shows that regardless of the recodings, the substantive conclusion remains the same—*no significant difference* (see Bishop, Tuchfarber, and Smith 1993, tbls. 3a–b).

7. A Z-test of the difference between correlations showed the following to be significantly higher with the *Michigan* form: (1) U.S. Senate vote preference in the April 1991 Ohio poll, (2) the favorability rating of Bush in the October Ohio poll, and (3) the comfort rating of Clinton in the same October Ohio poll, and (4) U.S. Senate vote preference in the same poll. The following were significantly higher with the *Gallup* form: (1) comfort rating of Clinton in the 28 October–1 November 1992 Ohio poll, (2) presidential vote intention (Bush vs. Clinton and Perot in the same poll), and (3) consumer confidence with respect to the outlook for U.S. business in the May 1993 Ohio poll. No pattern appears to exist in these significant differences beyond chance fluctuation.

8. Copies of these and other questions are available, upon request, from the authors.

9. The participants in this debate have also overlooked an equally plausible source of the variability among the different time series for the party identification question, namely, the influence of responses to previous questions in the interview—what we call "context effects." A casual inspection of the interview questionnaires on which these different survey time series are based will show considerable variability in the content of relevant questions preceding the party identification item, not only across organizations (Michigan, Gallup, NORC GSS, and CBS News/*New York Times*) but also within the same organization over time (see any of the cumulative codebooks for these surveys). The apparently greater variability of the relatively frequent Gallup (and CBS News/*New York Times*) polls may thus reflect their more rapid turnover of topics, shifts in responses to preceding questions about the most important problem(s) facing the country, presidential approval, candidate trial heats, and the like, as compared to the more slowing evolving, less variable contexts of the NES and the NORC GSS (see the cumulative codebooks). This explanation would fit with Abramson's and Ostrom's observations of the variability among the different time series, while allowing us to reject their rival question-wording hypothesis. Furthermore, having ruled out "house effects" as an alternative explanation with our in-house set of experiments and replications, there are hardly any other plausible, contending explanations left but context effects. (For a recent perspective of this literature, see Schwarz and Sudman 1992.) Indeed, the plausibility of the *variable context hypothesis* has grown with the recently published report by Johnston (1992) demonstrating, experimentally, that responses to party identification questions are affected by wording, depending upon the order and context in which they are asked. (On how the order and context of candidate preference questions may account for apparent variability in poll results, see also Frankovic 1992.)

10. We have also compared the *variance* of the responses to the two forms in each of our 15 experiments and found no significant differences in any of them nor any pattern of differences (data not shown here). If the phrase in the Gallup form "In politics, as of today" makes it somehow more responsive to short-term events at any given point in time, we would expect to see some evidence of greater variability. But none exists, making the Gallup responsiveness hypothesis even less plausible.

11. See Green and Schickler 1993 for an analysis of this survey. According to our recalculations of results they present (1993, tbl. 8), 15.5% of 1,489 respondents who were asked both the Gallup and Michigan SRC partisanship questions responded differently to them.

12. In an earlier version of their study, Bishop, Tuchfarber, and Smith (1993) discuss our panel analyses. In the present critique, however, they do not mention that we conducted a four-wave panel.

13. For the second and fourth quarter of each year, MacKuen and his colleagues rely upon a single Gallup survey. For most of the period they study, the Gallup organization conducted three to six polls per quarter. Bear in mind, however, that the variable context hypothesis has *not* been supported by any direct experimental evidence. If MacKuen, Erikson, and Stimson's results were replicated by using all available Gallup surveys (as aggregated quarterly), these findings would render the variable-context hypothesis implausible. Contextual variation may account for more "noise" in the Gallup time series, but it is difficult to see how it can account for the systematic tendency of Gallup macropartisanship to be more strongly related to short-term change in attitudes toward the economy than are short-term changes in Michigan SRC macropartisanship.

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NOTES FROM THE MANAGING EDITOR

FORTHCOMING IN MARCH, 1995

The following articles, research notes and controversies have been tentatively scheduled for publication in a forthcoming issue.

Charles O. Jones. "A Way of Life and Law." 1994 Presidential Address.

Larry Arnhart. "The New Darwinian Naturalism in Political Theory."

Kathleen Bawn. "Political Control versus Expertise: Congressional Choices about Administrative Procedures."

Robert D. Brown. "Party Cleavages and Welfare Effort in the American States."

Gregory A. Caldeira and James L. Gibson. "The Legitimacy of the Court of Justice in the European Community: Models of Institutional Support."

Dana Chabot. "Thomas Hobbes: Skeptical Moralist."

Jorge I. Domínguez and James McCann. "Shaping Mexico's Electoral Arena: The Construction of Partisan Cleavages in the 1988 and 1991 National Elections."

Milton Lodge, Marco R. Steenbergen, and Shawn Brau. "The Responsive Voter: Campaign Information and the Dynamics of Candidate Evaluation."

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James C. Clingermeyer and B. Dan Wood. "Disentangling Patterns of State Debt Financing." A research note.

Richard L. Hall and Robert Van Houweling. "Avarice and Ambition in Congress: Representatives' Decisions to Run or Retire from the U.S. House." A research note.

Bradford Jones, Benjamin Radcliff, Charles Taber, and Richard Timpone. "Condorcet Winners and the Paradox of Voting: Probability Calculations for Weak Preference Orders." A research note.

Randall D. Lloyd. "Separating Partisanship from Party in Judicial Research: Reapportionment in the U.S. District Courts." A research note.

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Christopher M. Duncan; Shelley Burtt. "Civic Virtue and Self-Interest Rightly Understood." A controversy.

Mary P. Nichols; Robert C. Bartlett. "Aristotle's Science of the Best Regime." A controversy.

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CONTESTING CARE

JEAN BETHKE ELSHTAIN *University of Chicago*

The Problem of the Passions: Feminism, Psychoanalysis, and Social Theory. By Cynthia Burack. New York: New York University Press, 1993. 140p. $35.00.

Disputed Subjects: Essays on Psychoanalysis, Politics, and Philosophy. By Jane Flax. New York: Routledge, 1993. 183p. $49.95 cloth, $15.95 paper.

Feminist Morality: Transforming Culture, Society, and Politics. By Virginia Held. Chicago: University of Chicago Press, 1993. 285p. $42.00 cloth, $17.95 paper.

Moral Boundaries: A Political Argument for an Ethic of Care. By Joan C. Tronto. New York: Routledge, 1993. 226p. $49.95 cloth, $16.95 paper.

Gendered Community: Rousseau, Sex, and Politics. By Penny Weiss. New York: New York University Press, 1993. 189p. $40.00.

Feminist theory is a moving target, difficult to stabilize. Indeed, for many feminist theorists—especially but not exclusively those associated with post-modernism—any attempt to fix the object of feminist discourse and to refine that discourse definitively is a dangerous aspiration tethered to discredited meta-narratives of logocentric mastery. ("The Enlightenment" usually comes into play here as the moment when such architectonic theoretical ambition crystallized for Western thinkers.) Other feminist theorists, however, believe that stabilizing the discourse is, or should be, a central ambition of any feminist theory worthy of the name. To do less, they insist, is to shrink from necessary philosophical tasks and exigent political actions. There are, of course, feminist thinkers who find themselves unable to go along with either post-modernism or its strongest discursive opponent and who prefer instead to deal competently and modestly with reinterpreting texts or pushing for an achievable set of political objectives.

The books under review offer examples of each *genre* and help to give a sense of the scope and diversity of current feminist scholarship. Whoever hopes to settle "once and for all" what feminism is about will scarcely be comforted by the sharpness of the issues joined here. Much of the most withering fire by feminist thinkers at this point is reserved for discursive and political opponents *within* feminism. To some this is a source of concern, even sadness. To others (and I include myself in this second camp), argument is what it is all about. After all, feminism historically has been defined in part by contests generated over its meaning. The diverse history of feminisms in the plural forms the basis of current feminism discourse and debate. Feminists can be found who push for the achievement of an egalitarian society, but there are others who, in the words of Jane Flax, doubt "that equality can mean anything other than assimilation to a preexisting male norm" (p. 113). There are feminists who embrace a strong notion of women's difference and others who minimize distinctions between the sexes.

What is being contested in the five books under review? Just about everything, save the need for feminist theory and a feminist-inspired politics, it seems. But a number of key issues surface again and again. They are perhaps best summarized in the form of questions: What is the actual or desirable relationship between political and social theory and political action or public policy? Is there a uniquely feminist way of construing this relationship? Is there a distinctive feminist mode of moral theorizing? Is there a distinctively female mode of moral thinking? Are women, in fact, more *caring* than men? If so, what does this mean and how does the felt imperative to care arise? Should feminists evoke maternal imagery as a symbolic representation of a worthy moral and political ideal? What of the *self* itself? Is there a *self* that can be pointed to or achieved, and can this attained or achieved self serve as the ground for political action? Does this self, whether "unitary" or "provisional," have anything to do with citizenship? I shall cluster these questions into three broad themes and take each up in the texts in question: (1) Feminist and female moral thinking and the question of care, (2) The self and politics, (3) The relationship between feminist theorizing and political action.

One way or another, each author either endorses or challenges what might be called the "turn toward care" associated with the work of Carol Gilligan and Sara Ruddick, among others. For Cynthia Burack, care has been reconceptualized by feminists as part of "the feminine capacity for nurturance, love, empathy" (p. 2). But she fears that an "analytic gap has emerged within feminist theory," insofar as feminists have failed to investigate fully gender identity. That identity has something to do with what Burack calls "the disagreeable passions," such as rage and hatred (pp. 3, 5). Feminist theory would benefit, she insists, if it took seriously these passions rather than persist in maintaining an untheorized ambivalence toward them. Burack would "reintegrate" the disagreeable passions, but it is never really very clear what this means or what such a reintegration would help us to attain—other than a presumptively more realistic assessment of human capacities. Burack, critical of Gilligan for an overemphasis on empathy, care, and love, puts her own money on object relations theory, as embodied in the work of Melanie Klein. Reiterating her basic theme, Burack claims that an integration of the "disagreeable passions" would help to complete a feminist "transformative vision of selfhood and social relations" (p. 23). We are told that these unsavory passions "are a feature of social and political life" (p. 46). But we are not offered a robust account of what features of social life the disagreeable passions might have a central role in constituting. Burack's critique of Gilligan for her one-sidedness is not as compelling as it might be because her call for an account of the "disagreeable passions" is not followed by such an account. We are asked to join Burack in the hope that a reconstituted self—in the form of a "self-critical feminist object relations theory"—would offer not only a more complete view of the self but a more compelling vision of feminist theory in relation to social transformation, as well. An agreeable book, clearly written, Burack lays on the "we need to's" pretty thick, but the book is thin on the ground when it comes to showing what integrated "disagreeable passions" would look like in political life, that is what form or forms these might take. Concrete examples of what can be laid on the doorstep of an unintegrated vision of self (one that denies the "disagreeable passions") by contrast to the preferred integrated self who has come to terms with these passions would have made for a more compelling volume. I think of war, for example, as a pervasive human institution. Does this arise from integrating or failing to integrate the "disagreeable passions?" Burack is surely correct that no political and social theory in this day and age is likely to persuade unless some mention is made of the "self" or the "inner life." But she has failed to specify how a reconstructed self molded on the object-relations model might think and act politically in ways significantly different from the old unintegrated self.

At first glance Penny Weiss's analysis of the theme of gender in Jean-Jacques Rousseau would seem to have little to do with the concerns that currently preoccupy feminist thinkers. Indeed, her book starts out rather traditionally (if the recent history of feminist political theory is one's benchmark.) She acknowledges that the ground has been well been covered in the matter of Rousseau and she makes the pitch—one not unfamiliar to readers of earlier treatments of Rousseau on women and politics—that one cannot hive off bits from Rousseau's political texts and sever these from such works as *The Emile.* Fair enough. Weiss's own take is that Rousseau is internally consistent in this sense: sexual politics is central to his work throughout. And she also makes a strong pitch (her second aim) for why it is important to continue to study "major" (she puts this in scare quotes) historical figures in political philosophy. Her treatment of Rousseau is admirably balanced, particularly her insistence that the ostensibly free Emile is in fact as "manipulated by his education" and his condition is as "marked by limitations" as that of Sophie (pp. 25, 29). Restraint, for Rousseau, is what it is all about. Thus Sophie is no more a "helpless slave" than Emile is a "tyrant" for each is required by Rousseau to see her or himself "as parts of larger wholes, to which they contribute and from which they receive necessary and desirable benefits" (p. 32). Well and good.

How, then, does Weiss join the issues I have located as organizing themes of the texts under review? In the following way. She insists, first, that there is "good reason to be suspicion of the creation and enforcement of systematic differences." In other words, she comes down on the side of equality, fearing that "difference" is "often nothing more than a palatable name for inequality." And yet she does not want to efface difference entirely. Some differences, "such as cultural ones, can be enriching. Assimilation and uniformity are clearly not always to be preferred" (p. 35). How to promote good differences and forestall bad inequalities is not made clear, although Weiss makes the welcome point that feminists "should not be too quick to point to any role difference as proof of inequality" (p. 74).

At this point, the wheels begin to grind a bit and the joints to creak. Despite her riposte concerning role difference, Weiss concludes that "sex roles, in fact, are self-defeating and illegitimate" (p. 89). The strong term of opprobrium—illegitimate—seems to come out of nowhere at this point as does Weiss' conclusion that equality "requires abolishing the political categories of 'woman' and 'man' " (p. 91). I am unsure how much slippage in the argument is going on here because Weiss does not explain what the *political* categories of *woman* and *man* have to do with biological femaleness or masculinity; how we might "condition" children to respect difference but not to be "political" men or women; nor what these categories have to do with an ideal of citizenship available to both men and women in a democratic society.

She tips her hand in the direction of Carol Gilligan, but it is difficult to endorse Gilligan and to call for elimination of the categories of man and woman at one and the same time. Her final word on Rousseau is that he offers "no convincing account of the unhappiness of women who are fully immersed in traditional gender roles and family structures" (p. 115). She does not consider the possibility that unhappiness just may be the human condition. What about men "fully immersed in traditional gender roles?" The male tradition of going to work every day, day in and day out, year after year, in order to support a family probably did not yield giddiness. A final problem with Weiss' texts is that, in the second and weaker section of her book devoted to "Feminism and Communitarianism," she decocts feminism to one conception of the self and sees this feminist self as a clear-cut alternative to "abstract individualism." But surely this bumps up against her insistence that *man* and *woman* as categories must be effaced, particularly as she goes on to emphasize the ways in which relations between mothering persons and children—"characterized by more care and concern and openness and trust and human feeling"—should serve as models for *all* relationships (p. 137). Would she include citizenship here? Rulers and ruled? Or would the latter be eliminated in a future caring world?

I will take up Weiss' call for an "unbiased political theory" at the conclusion of this review, but let me note one unfortunate lapse. Given her expansive and admirable interpretive generosity toward Rousseau, it is unfortunate that Weiss blindsides Nietzsche with one curt interrogatory and one snappy injunction from *Thus Spake Zarathustra,* to wit: "Do you go to women? Do not forget the whip!" To which Weiss replies bitterly: "Thus spake misogyny." My question is: Why should Nietzsche be exempt from the interpretive complexity Weiss brings to bear in the case of Jean-Jacques? Do we have different hermeneutical rules for different thinkers?

Virginia Held, a distinguished feminist philosopher, belongs solidly in the camp of those who see post-modernism as a threat and who yearn for a world-view that aspires to completeness and serves as a clear and direct goad to transformative social action. This is an admonitory volume, dominated by "shoulds" and "oughts," relentlessly injunctive. Held claims, first, that feminism is "transforming the ways in which we think and live and express ourselves" and, second, that feminism must go even further, or feminist philosophy must, in offering up a general understanding "of reality, knowledge, methods of inquiry, and morality" (p. 16). The subject and object of her discourse is a *we.* "We often address . . . We often argue . . . We look to culture. . . ." She admits that feminists "are . . . not a unified group. We all belong to other groups, such as those that make our racial or class or ethnic backgrounds, and thus our experiences, very different" (p. 1). This is a bit odd on the face of it. Is being a member of something as amorphous as a "race" or a "class" a form of membership in any coherent sense? Held assumes so but does not argue the point.

I will reserve her strong tethering of feminist philosophy to direct action for my conclusion but it should be noted that Held is convinced that postmodernism sends feminism off the rails because it contributes "to a loss of political activism among those engaged in women's studies. "This, too, is odd in several senses, first, because Held sees those laboring in the vineyard of women's studies as being on the forefront of feminist *political* action and anything that deters them from this mission must be gainsaid and, second, because she attributes such enormous political power to philosophy. Without a philosophic prod action withers, apparently. Thus it comes as no surprise that Held's ubiquitous *we* requires "detailed guidance, and we need to assemble arguments for our various recommendations" (p. 9).

When her *we* isn't going about this forbidding task, she (individually and collectively) must force moral theories to confront lived life and, as well, embrace an "approach to morality" that "might give a felt relationship of trust priority over principle and seek a morality compatible with this priority" (p. 33). In other words, the "felt" relationship is the tail that wags the principled dog. Held has high hopes for what might result, nothing less than an alternative to moral theories "constructed from male points of view," hence not "gender neutral" (p. 43). Care enters as that which is to be embraced and that which feminist moral theory celebrates and locates as central. "Caring, empathy, feeling for others, being sensitive to each other's feelings—all may be better guides to what morality requires . . . than may abstract rules of reason or rational calculation," Held writes (p. 52). She accepts, with no hint or skepticism, Gilligan's "different voice" ("we already have evidence") and she claims that men have had "fewer occasions for experiencing the moral problems of family life" than have women (p. 69). On the face of it, this is a rather extraordinary claim but it goes undefended.

Held places a tremendous moral and political load on the "activity of mothering." Although, in a somewhat off-handed way, she suggests that the activity of "mothering" (is there, then, to be no more "fathering"?) is as open to men as to women, men are quickly effaced as Held avers that it is women who "are the persons who create other human beings. . . . Men produce sperm and women produce babies, and the difference is very great" (pp. 80–81). It is unclear what sort of claim this is other than a polemical one. Are there ontological implications? Ethical concerns? It would seem so for Held goes on to make, and then to repeat, a claim along these lines: Women suffer "severe pain for the sake of new life." This suffering leads to a "natural tendency to value what pain has been endured for" (pp. 82–83). Her repetition of this melodramatic claim is even worse, by which I mean more suspect from a moral point of view, as it makes the child's "value" contingent upon a woman's suffering. She insists: "The reason, if it is one, that the child should be valued because I have suffering to give the child life is different from the reason, if it is one, that the child should be valued because someone unlike me suffered to give the child life" (p. 85).

On this shaky foundation, Held hopes to build a feminist morality within which the flourishing of children would be central. She lashes out against the mass media as a perverting influence; offers up a short list of human rights that locates "freedom of expression" near the top of the list; repeats the injunction ("we need . . . we will need") to create theories of social change and to redesign institutions. But when it gets down to brass tacks, Held's own "redesign" is strikingly thin. For example, her three page discussion of "Violence, War, and Politics" nowhere comes close to coming to grips with the pervasiveness of organized, rule-governed violence. Nation-states are bad, we learn, because they are built on a willingness to use violence; politics itself is suspect because it is but an extension of the "male barracks community" of "ancient warrior-heroes," an ahistoric formulation that cannot even account for the profound shift from the Homeric age to that of settled polis life—let alone anything beyond. Held segues from this fable of one long unbroken tale of warrior-heroes to the curt suggestion that men think of their children as "replaceable." Men may be "naturally" deficient in this regard, being "more indifferent to particular children than has been considered." Held produces no evidence to sustain this harsh indictment. There is a vast literature expressing fatherly love and devotion that one might take up before saddling men with a noxious indifference to their particular children.

Having trotted out the word "naturally" several times, it comes as a bit of a surprise when Held excoriates any suggestion that women are "more immersed in biological processes, more determined by instinctive and other 'natural' forces than men" (p. 151). At this point Held becomes something of a social constructionist, one who wants to begin with the "point of view of women" and to reconceptualize an entirely new society with "the dynamic relation between mothering person and child" as the "primary social relation" (p. 213). In this way and this way alone will humanity finally reject domination and attain "harmonious relations" between human beings and the global environment (pp. 225–226). What such harmonious relations would look like we are not told but, then, since "every domain of society needs transformation, from the military, law, politics, and economic activity, to cultural production, education, the health and welfare systems, and the family," we need not hold our breaths (p. 214). We'll be lucky to get health care and welfare reform any time soon so utopia is rather far off.

Jane Flax's defense of a psychoanalytic, post-modern feminism serves as a direct rejoinder to Held's project. Flax calls the hankering for a "unitary representation" a wish "to deny or avoid the many conflicts among women" (p. 27). She is open to those "disagreeable passions" of which Burack writes. Her perspective is of one deeply pragmatic ("We adopt the knowledge that fits our uses") and suspicious of "confidence in the integrity and powers of reason" (pp. 31, 52). Psychoanalysis is central in challenging various Enlightenment "ontological premises" and "positing various forms of desire . . . rather than reason as the definitive and motivating core of our being." Flax's self is provisional, pleasure seeking, alive in a world of subjectivities yearning "for multiplicity" which "can impel them toward emancipatory action" (p. 93). Indeed, attention to the process of gendering itself "reveals the delusory character of self-determination, individualistic, and autonomous ideas of subjectivity" (p. 97).

There is a good bit of repetition in Flax's text, a series of essays thematically united by her brief in behalf of "multiplicity, randomness, and contingency" by contrast to "order and purpose" (pp. 99–100). Although her rejoinder to high Enlightenment hopes (that "reason" will make us "free") seem to me well taken, *Disputed Subjects* is a bit frustrating from time to time as so much of it is devoted to questions, sometimes entire pages. We learn that we are not to deny our own pleasures, including expressions of aggression. We learn, and it is an important lesson to learn, that no one no where at any time is simply an "innocent victim(s) outside circuits of power" (p. 68). But how does this play politically? Flax works on this question by challenging the "valorization of maternal images within contemporary feminist theorizing and the splitting of (good) maternity and (bad) sexuality" (p. xi).

For Flax, the maternal turn in feminism is suspect; it is "neither accidental nor innocent" (p. 59). To be sure, it is not and cannot be "innocent." No endorsement in this world is or should pretend to be. Flax makes this case well. But she is unconvincing in suggesting that there is something of a proto-conspiracy at work in the simultaneous emergence, on her reading, of maternalistic endorsements in feminism and the disruptive coming-into-view of "women of color," lesbians, and others who do not fit the "white women" feminist paradigm. If, as she insists, all knowledge is "fictive" and "non-representational," we need more instruction on how to simultaneously endorse (anything) and make claims in behalf of one's own claims. Learning that citizenship "is also another transitional practice that helps manage the strain between the subjective and objective worlds" isn't much of a help on this score, first, because it turns on the sort of binary poles Flax objects to quite strenuously and, second, because the public world or public life come down to those arenas within which I transform my "I want" into "I and others in my situation are entitled" (p. 125). But doesn't this mean politics, or anything that Flax would acknowledge as a political process, must *always* be built on wants—from "I want" to "we are entitled?" Isn't that, in fact, pretty much what we have and one reason so many despair of contemporary American politics?

Although Flax is on target in warning against the use of "parent-child relations as a direct model for citizenship or political action," her understanding of politics seems deficient in articulating a political "we" that is something more, or other than, an aggregate of "I want's." Enter Joan Tronto whose *Moral Boundaries* is the most solidly and emphatically *political* of all the texts under review. Tronto would probably not escape Flax's brief against the "valorization" of care, but Tronto is no sentimentalist. Tronto hopes to re-situate care, framing it as hard hands-on work undertaken variously by women and others, and being "distributed by gender, by caste and class, and often, by race and ethnicity as well" (p. x). Tronto is unabashedly in favor of a "liberal, democratic, pluralistic society," a refreshing and clear endorsement by contrast to, say, Held's more tepid and somewhat perplexing insistence that "feminist society would be likely to have democratic political processes" (p. 224).

Tronto accomplishes a great deal in her wonderfully well-written, cogent book. She calls attention to boundaries between morality and politics; between the moral point of view and alternatives to such; and between public and private life. She does not want to abolish the distinction between public and private but to consider how, thinking both strategically and in more visionary ways, these and other boundaries can be rethought and redrawn as part of a democratic, contested political process. Rejecting both deontological universalism and post-modern feminist theory, Tronto stays on the ground of practical reason. She parts company with feminist theorists who either despair of liberal democratic societies or articulate romanticized alternatives to them. She argues in behalf of a political concept of care with care defined as "everything we do to maintain, continue, and repair our 'world' so that we can live in it as well as possible" (p. 103). Refreshingly, she sees strong antecedents to "women's morality" and a care perspective in Scottish Enlightenment thinking with its emphasis on sentiment and its locating of the household as an antidote to the corruption and vanity of the public world.

Rather than assuming that men and women have different senses of morality, Tronto asks why this question is so interesting to us. This is a very good move by contrast to a too hasty endorsement or dismissal of the idea. Her response is complex and distinctive. Because morality is "always contextual and historicized," it would be surprising it were not "deeply gendered" (p. 61). But that in and of itself does not tell us very much. In fact, the Kohlberg-Gilligan debate repeats rather than reshapes the boundaries within which we take up this matter. Tronto offers one of the strongest critiques of Gilligan I know, insisting that Gilligan's "different voice" isn't gendered in the way Gilligan claims and, moreover, that it winds up being an "account of partial privilege, not . . . an account of an alternative way to conceive of morality" (p. 63). Tronto accounts for why the Gilligan view has been so widely embraced by arguing that this version of gendered morality helps to preserve the distribution of power and privilege along a number of lines—gender, class, race, ethnicity, and education, among others.

A political concept of care, by contrast, one revolving around care as a complex practice, taps at one and the same time "universal" imperatives and "particular" commitments and helps the theorist to raise a variety of questions about the quality of care in any given society. Tronto doesn't want care relegated to the world of obligatory moral theorizing—a world of "ought's" and "owe's." Rather, she recasts the ethic of care along these lines: "How can I (we) best meet my (our) caring responsibilities?" This version of care is viable as a political ideal only in the context of democratic institutions. There are, to be sure, dangers in the care perspective, including maternalism and parochialism. I wish Tronto had said more about these dangers. In fact, it seems to me that one of her political endorsements—formal day-care as a policy provision of the United States government—might be called into question under her own care ethic. Many are uneasy about formal government-sponsored day care not because they don't care about care but because they worry about standardization, bureaucratization, and excessive "distancing" between care giver and receiver. Tronto's contrast between "private" day care, which she regrets, and her endorsement of public care on the grand scale is misleading. We know from reputable studies done on the subject that most mothers want in-community or in-neighborhood care; they want it to be small and personal; and they want it located in churches or other "spaces" with which they are familiar or to which they have some commitment. This is a complex mix of private and public imperatives rather than one or the other. It would, as well, be interesting to learn from Tronto what she takes to be the limits of the care ethic: when does care break off and punishment or severing of ties take over or become necessary?

Finally, a few words about theory and its connection to "experience" and to political action. Tronto has the most nuanced view, showing the ways in which our ideas and commitments enter into our political actions but in a way that is complex, not straight-forward. Burack has less to say on this topic but she, too, assumes the importance of theory *for* politics. Weiss wants nothing less than an "unbiased political theory," one that she defines as knowing "that the personal is political and the political is personal" (p. 160). This would exempt many thinkers, including Hannah Arendt, from the world of "unbiased theory," for Arendt not only *knew* no such thing, she criticized it explicitly. Held is most adamant in insisting that philosophic transformation *must* underscore political action, finding, as she does, a direct line from "understanding from a feminist point of view" to "making recommendations" for "more suitable social arrangements" (p. 4). This is not a point Held argues; it is one she asserts. Flax, on the other hand, severs the connection pretty completely when she moves from an initial claim that there is no "straightforward" relationship between "theorizing and experience" to a later insistence that it is altogether unclear what contributions "knowledge or truth can make to the development of . . . feelings and communities" (p. 147).

It might be intriguing at this point to put a Tronto-like question: Why do political theorists and philosophers continue to find it essential to justify their thinking with the notion that there is, or must be, some "pay-off" in the world? What happens when one makes an irenic move of Flax's sort and punts on the question? And, as badly battered as it is, might there not be something to the ideal, not so much of neutrality, as of *impartiality* as Arendt understood it, an impartiality that entered the world when Homer wrote that if the songs of the vanquished were silenced, "I myself will testify for Hector." For Arendt, any thinker or historian or theorist worth her salt must be one capable of such impartiality. This is an aspiration, or a role Arendt called that of "the guardian of historical truth," thus far conspicuous by its absence in feminist theory. Can there, in fact, be a *feminist* theory that embraces this Arendtian ideal? This might be an interesting turn for feminist discourse to take but I doubt very much it will happen, not so long as we remain pinioned in between calls to strenuous battle, on the one hand, or to the play of multiplicities of subjectivity, on the other.

POLITICAL THEORY

Recasting Conservatism: Oakeshott, Strauss, and the Response to Postmodernism. By Robert Devigne. New Haven: Yale University Press, 1994. 268p. $28.50.

Michael Oakeshott and Leo Strauss have done as much as anyone in this century to revive political philosophy as a substantive field of inquiry. In very different ways their collected works mark a standing refutation of the obituary for political philosophy pronounced *inter alia* by Peter Laslett in the first series of *Philosophy, Politics and Society* (1956). Yet despite their preeminence in their chosen domains, there has not been, until now, a comparative study of their thought and influence. Robert Devigne's book represents a first step in that direction.

Recasting Conservatism is, as its title suggests, an exploration of ways in which Oakeshott and Strauss have shaped recent developments in British and American conservative thought and, to a lesser degree, policy. It may come as something of a surprise that while both thinkers eschewed the role of public intellectuals and explicitly warned against confusing philosophy with political action, they are seen here as purveyors of powerful and influential brands of conservative ideology. There is the additional irony that neither Oakeshott nor Strauss thought of themselves as conservatives in any ordinary sense. Oakeshott's thought displays marked affinities, not with Burke, but with Hobbes, perhaps the least traditional of English political thinkers. Strauss often emphasized the degree to which philosophy as such is predicated upon a break with traditional sources of opinion and authority. To be sure, Devigne acknowledges that conservative opinions and beliefs have been shaped by many non-theoretical sources as well. Nevertheless, he finds evidence that their views have come to be widely shared by those occupying positions of power in both Whitehall and the U.S. State Department.

The most interesting parts of the book are the contrasts it draws between the British and American styles of conservatism. Recent British conservatism has championed a revival of the state as the sole law making body and locus of sovereignty. Devigne interprets Oakeshott's magisterial *On Human Conduct* (1975) as the opening salvo in the conservative defense of the state as the authoritative propounder of law. Unlike the older conservatism with its defense of the "small platoons" (Burke) of society, the new conservatism, drawing on the authority of Hobbes, Bodin, and Hegel, has called for an undivided national sovereign that would put limits on alternate sources of authority often stemming from labor-dominated local councils.

In the U.S., by contrast, the new conservatism has pursued a twofold strategy. On the domestic front, conservatives have generally defended the mediating institutions of civil society as the proper locus for the cultivation of citizen virtue, for only at the local level do there exist opportunities to foster such virtue effectively. Yet at the federal level, they have favored a powerful, even imperial, role for the presidency in asserting a unified national interest. Devigne sees Strauss's *Thoughts on Machiavelli* (1959) as advocating a "poetic idea of greatness" as a model for executive action, especially in the struggle against Soviet communism. Straussian conservatives, then, have tried to revivify a concern for religion, public morality, and citizen virtue at the local level and a poetic desire for fame and glory at the national and international levels.

The difference between these two conservatisms can to some degree be traced back to Oakeshott's and Strauss' different readings of Hobbes. For Oakeshott, *Leviathan* represents the argument for the strong state combined with the free economy and private liberty. Oakeshott's Hobbes is not the defender of monarchical absolutism, but the theorist of "civil association" understood as a kind of pure procedural formalism. The purpose of sovereign authority is not to advance a national agenda or serve some common purpose but to lay down laws which prescribe certain "adverbial conditions" to be subscribed to in conduct. Problems arise when one confuses the state with some kind of "enterprise association" aimed at the attainment of some overriding end or purpose.

For Strauss, Hobbes' psychology of fear and pride almost perfectly expresses the dual model of governance that Devigne attributes to Straussian conservatives. These two dispositions not only represent two types of political ambition but two opposing social and psychological types. At the same time that Hobbes made the fear of violent death the universal passion that would underwrite his new egalitarian social order, he recognized that the aristocratic passion of pride ("vainglory") could not be altogether eliminated from human nature. The task for the Hobbesian statesman was to domesticate as far as possible the honor-loving aristocrat so that he might be made useful for enlightened or peaceful ends. Devigne cites Joseph Cropsey's seminal "The United States as Regime" to the effect that from its founding American politics has sought to find institutional means for balancing the demotic need for safety, security, and survival with the heroic desire for greatness and resplendent glory.

There is much in this book that is useful and enlightening. To understand political theory by examining its influence on broader intellectual and policy debates is a legitimate and worthwhile enterprise. However, to reduce thinkers of the stature of Oakeshott and Strauss to their influence (real or alleged) on the formation of public policy is to risk misinterpretation. One can virtually comb the collected works of both authors and find scarcely a word on current events and policies. Their writings are in fact rife with warnings against using political philosophy as an instrument for changing the world. Oakeshott explicitly distinguished political philosophy of the kind found in Plato's *Republic*, Hobbes' *Leviathan*, and Hegel's *Philosophy of Right*, which is necessarily thinking *sub specie aeternitatis*, from political ideology, which has the task of justifying and defending actions in terms of general principles or "isms." Likewise, Strauss distinguished between certain writers' esoteric and exoteric communication as a way of distinguishing true philosophy from the merely salutary opinions that a writer might from time to time espouse. Like

Oakeshott, Strauss warned that the politicization of philosophy necessarily leads to its corruption.

In treating Oakeshott and Strauss as conservative philosophers one necessarily puts philosophy into the service of a political cause. This is to miss the profoundly skeptical disposition of their philosophical politics. In one of his most beautiful expressions, Oakeshott called himself a skeptic who would do better if only he knew how. Similarly, Strauss described his philosophy as fundamentally "zetetic" or skeptical in the Socratic sense. The task of philosophy is not to offer solutions to current or timely problems, but to keep alive an awareness of "the fundamental and comprehensive problems." In his exchange with Alexandre Kojeve, Strauss remarked that "the sectarian is born" at the precise moment when a belief in our own certainty comes to outweigh an awareness of the problematic status of our beliefs. Philosophical skepticism may go hand in hand with a certain type of conservative inclination to maintain existing social and moral practices. In fairness, Devigne makes some effort to distinguish between their intentions and the views of those who have been influenced by them. But unless one grasps the fundamentally skeptical temperament of Oakeshott's and Strauss' philosophies, one will understand nothing of their politics.

In addition, there is the problem of paying Oakeshott and Strauss the honor of attributing too much political influence to their writings. Oakeshott's influence on British politics came largely through John Gray, Shirley Letwin, and the contributors to the *Salisbury Review* who had some sympathetic readers within the successive Thatcher governments. A number of Strauss' students (or students of his students) went into government service during the Reagan and Bush years, although at least one is currently a well-placed official in the Clinton administration. But it is unclear to what extent Oakeshottians and Straussians actively shaped the conservative agendas of their respective governments or simply took advantage of them. Harvey C. Mansfield's acerbic barb that Straussians went to Washington only after they could not find university jobs seems to indicate that the desire to change policy was not the career of choice. One could also add Dan Quayle's remark that he tries to read Plato's *Republic* every year both as evidence of the influence of Straussians in the Vice-President's Office and as vivid testimony to the limits of theory.

Recasting Conservatism is a book well worth reading for anyone interested in the relation between high theory and certain lower order political debates in the U. S. and Britain. The author's treatment of Oakeshott and Strauss is fair and even-handed throughout, neither defensively partisan nor angrily recriminatory. If I have been less than whole-heartedly enthusiastic in my endorsement, it is because I doubt that any work, however well-crafted, could render complete justice to the weight and complexity of this subject.

Yale University STEVEN B. SMITH

Francis Bacon and the Project of Progress. By Robert K. Faulkner. Lanham: Rowman & Littlefield, 1993. 308p. $55.00 cloth, $21.95 paper.

The Francis Bacon in Robert Faulkner's book is a far more radical thinker and politic writer than one is likely to expect. Faulkner's Bacon is a man whose ambition went beyond teaching humanity the know-how needed for operating masterfully on nature: he had a plan for operating craftily on human nature to produce modes and orders as new as could be. Faulkner makes the argument clearly and with a deep appreciation of fundamental alternatives in political philosophy. His comprehensive treatment of the Baconian alternative gives particular attention to the *Essays or Counsels, Civil and Moral*. While bringing out Bacon's artful indirection as an author, Faulkner provides many straightforward summaries of his own argument, so that the argument itself will be easier to follow even though the evidence cannot avoid being crabbed and even seeming strained. At times Faulkner may go too far—but perhaps not; and it can be said with confidence, even prior to the assiduous work of checking out Faulkner's many interesting interpretations, that he has done a great service by drawing upon close reading in a vast and multifarious body of texts and presenting clearly the argument that modernity was, to a surprisingly large extent, prepared as a project in the writings of Francis Bacon. To establish Bacon's originality and influence is not Faulkner's concern; rather, it is to grasp the scope of Bacon's plan and to judge the adequacy of its premises. Postmodern disillusionment with the progress of modernity can lead to nihilism, but it should lead to rethinking of the disillusionment at the root of modernity. Faulkner intends his book to contribute to such rethinking, and it is an excellent contribution.

Part I, on "the politics on enlightenment," opens with the thesis that Bacon had an extraordinarily comprehensive plan. His famous project of a new science for instituting human power over nature, Faulkner says, is defined by his thinking on human affairs. Not only the applications of his natural science but also its end (human power) and the sort of knowledge that it seeks (experimental laws of operation or production) are governed by his doctrine of the "self." Man, for Bacon, is above all a self in need of power, comprising first the worldly skills that can advance the self-made man and then the state of progressive civilization that can supply security to peoples and to humanity at large. The plan's purpose and principles conclude this part of the book: Bacon's project of progress is a fundamentally Machiavellian edifice, modified by improvements (for private security, limited government, economic growth, and conquest of nature) that make it a more humane, more civil, and more visionary version of its nasty original. The stages of Bacon's plan are surveyed in this part's central chapter, which examines his rhetoric by keeping in mind his various remarks about secretive, indirect, and manipulative discourse. Although various writings by Bacon are treated at length, Faulkner's book emphasizes the *Essays*. Each essay, Faulkner says, takes traditional or familiar opinions and turns them into new, enlightened opinions while disguising the comprehensive civilizational transformation that Bacon is bringing on. His writing appears disorderly because he must operate indirectly in order to be effective. Using cunning toward his followers as well as toward his foes, Bacon manipulates fear and vengeful anger, acquisitiveness and ambition, in order to subvert the opinions and institutions that support clerics and nobles and even, in the end, the kings under whom the building of the modern state is to begin. Bacon's collection as a whole moves, like each essay in it, from critique to construction—going from subversion of the most authoritative received pieties at its beginning to production of a new-model, progressive nation–state in its middle to intimations of a new-model progressive civilization at its end.

The rest of Faulkner's book sets out Bacon's plan for the conquest of human nature through the making of the self-made man able to contend in an alien world (Part 2) and then for a man-made state of the world appropriate to such a self (Part 3).

Faulkner says, early in the book, that this last part, on "the state of progress," contains "the obviously important and political innovations" and that "the reader who wishes to reach quickly much of the meat should turn there first." In this part, Faulkner contends that Bacon's doctrine of the state is far from the Elizabethan monarchism with which it is often identified: rather, Bacon plans a nation–state of the type that we call modern—one bent on economic and industrial growth and presided over by a republican but effectual government. Such growth-oriented nations will constitute a world of great powers but will harbor Bacon's science with a human face. Each of Bacon's major political writings does its own work to produce the new orders, by showing how enlightened despots can establish a state, how rising men can transform an established state into a civil state (a civil society and a civil government), and how progressive intellectuals and their allies among the mercantile class can introduce into the civil state a visionary scientific project for ameliorating human suffering. Bacon follows Machiavelli in criticizing classical political philosophy as useless and in promising effectuality through reliance on the most powerful of passions; but while Bacon continues the building of hopes on fear, his own specialty is the nourishing of hopes.

Part 2 treats Bacon's transformation of ethics into a radically individualistic doctrine of the self-made man. Individualism is the central doctrine of modern politics, Faulkner says, and the life of the self is central for Bacon; his humane science, radically rethinking human ends and needs, discovers a needy self that must make its own provision to the point of making its own world. Bacon produces first a critique of divinity and morality as well as philosophy and then a construction of arts that enable the self to manage itself and others into the power that will preserve itself and them. Nature, even human nature, is stingy: the good that men get—even their purposes—they make for themselves. Morality is realistic humanitarianism, mixing a show of provision for humanity's needs with primary provision for satisfaction of one's own passion to survive. To serve oneself, there is need for public policy and also for a public persona as an image to win followers. Indeed, public persona is the delivery of self: while fear makes us wish to save ourselves, the art of doing so originates in vengeful anger at our natural state of misery, and real revenge is a far-sighted domination that gives the self a life after death through the devising of a public image; this culminates in the image of a new-model Caesar who will blend civil supports with promises of humane benefactions to achieve a general and durable conquest of the minds of future generations—with Bacon himself revered, in the end, as humanity's true savior.

The very last chapter of the entire book evaluates the premises of Bacon's project, subjecting his teachings, both on nature and on human nature, to the same twofold critique: his reductionist reasoning begins by excluding various naturally given notions that human living cannot do without; and his construction work makes use of an impoverished version of what his demolition work excluded at the outset. Faulkner criticizes the doctrine of productive experimentalism that is the scientific foundation of Bacon's work and the doctrine of the self-made man that is the moral core of it. Faulkner does not, however, criticize the Baconian modern civil order; Faulkner is a politic man, and that order is the regime under which we live. It is also the regime in which we prosper, and Faulkner is a humane man; the truly progressive achievements of the regime should be defended, Faulkner says. But, he argues, the Baconian foundation and its core will not provide the means to defend or even properly to estimate the achievements of the regime.

Where to look for help? In the middle chapter of part 2, Faulkner centers attention on Bacon's rejection of "*the* philosophical alternative"—the classical teachings on wisdom and friendship that Bacon sought to correct. Those who would rush into postmodernity, seeking an alternative to the doctrine that informs the Baconian project, can find, in Faulkner's book, reason to pause for a good long look back.

St. John's College, Annapolis HARVEY FLAUMENHAFT

Hegemony and Power: On the Relation between Gramsci and Machiavelli. By Benedetto Fontana. Minneapolis: University of Minnesota Press, 1993. 226p. $44.95 cloth, $16.95 paper.

Gramsci is almost the only communist leader to survive with honor in the "post" fields of so many recent currents of leftist thought. He has, in fact, not only survived but flourished, through the widespread dissemination of key concepts like *hegemony* and *organic intellectual*. Fontana notes early in his introductory chapter the multiple appearances Gramsci continues to make, depending on the interpreter: "the Crocean or Hegelian Gramsci, the Leninist Gramsci, the Jacobin Gramsci, the democratic–spontaneist Gramsci, the voluntarist Gramsci, and so on ad nauseam" (p. 2). The *ad nauseam* is rather too much like an expression of Crocean intellectual *distacco* for a book that otherwise takes very seriously Gramsci's insistence that philosophy must become a popular political force. After all, multiple Gramscis around is not automatically reason for contempt. It can mean that his ideas are caught up in, and inform, significant political conflicts of direction; and after this early and distancing gesture Fontana himself gets quickly to the business of interpretively pursuing the complications in Gramsci's analysis of Machiavelli.

Fontana's angle of entry is a crucial one, for while Gramsci has indeed survived with honor even in self-designated "post-Marxist" thinking, the sticking point remains Gramsci's modernist (no *post*) insistence on the revolutionary necessity of an organized, mass-based political party. And nowhere is that insistence more evident than in the notebooks concerned with Machiavelli. Laclau and Mouffe's summary comment in *Hegemony and Socialist Strategy* (1985) can serve as a typical "post" dissociation from at least this aspect of Gramsci's politics: "We will thus retain from the Gramscian view the logic of articulation and the political centrality of the frontier effects, but we will eliminate the assumption of a single political space as the *necessary* framework for those phenomena to arise" (p. 137, emphasis original).

I think it a fruitless debate whether one can thus pick and choose parts of a political philosophy or whether one has to swallow a system whole. Gramsci is not a system in any case; he is not medicine (good or bad) that one has to swallow; people will use what they find useful and discard the rest. The serious issue Fontana's analysis raises (if indirectly) concerns the "strategic" sense in such a summary dismissal of the relevance of party politics to a "post" world. Fontana's approach is indirect because focused, first of all, on a matter of interpretation: How exactly did Gramsci understand "political party" in relation to Machiavelli's thought?

For Fontana, Gramsci was interested in Machiavelli "as the thinker who first understood and first theorized the need for a politics that would recognize the presence of the 'mass' as a new factor in the power equation" (p. 8). Croce's response to Machiavelli's recognition was that if they cannot be kept out of the "power equation," they can at least be kept out of knowledge; hence one had better be sure that knowledge and power are clearly understood as two very different things. Machiavelli was about power, not knowledge. Fontana's claim, however, is that Gramsci's counterreading of Machiavelli is not only a challenge to Croce's attempt to keep philosophical knowledge and ethical direction distinct from politics as a purely "technical" field of power relations. More fundamentally, Gramsci's reading insists that Croce's very focus on that distinction is already a political displacement. What seems from Croce's liberal perspective both a threat and a mystery (What happens when the "mass" becomes "a new factor in the power equation"? is transformed into an intellectual dilemma that might then admit to philosophical resolution: What is the relation between knowledge and power? Croce's feat of prestidigitation is the assurance that if you can get the right answer to the second question, the first will solve itself and the power threat posed by "the mass" disappear. Gramsci's "anti-Croce" begins with Machiavelli, as a reminder that Croce was asking the wrong question.

Fontana's second and connected claim is more complicated and takes up much of the book in its elaboration. Briefly, the thesis is that if, unlike Croce, one *begins* with "the presence of the 'mass'" as a political factor, then one must find a way to understand politics otherwise than as domination. Thus, for Fontana, the figure of "the prince" in Machiavelli (as, correlatively, "the party" in Gramsci) does not mark the emergence of new types of political leaders and new forms of instrumental political power interests. It is, instead, a figure embedded in a process of social transformation that constructs the political order as a realm of freedom. The party as "modern prince" is then out "to teach the people a new political knowledge" (p. 151), Fontana allows. But he adds immediately that "this knowledge . . . must now be understood as one always internal to the people, because the democratic philosopher (the teacher) is himself a product of the people he intends to educate" (p. 151). The party, in other words, is neither a cadre of revolutionary leaders nor the intellectual representative of an already existent (if mute) "class subject." The party is the democratic philosopher produced by the people as they themselves becoming democratic philosophers in action. It is finally a figure of politics-as-freedom built out of the destruction of politics-as-domination.

Thus, like Laclau and Mouffe and other "post" theorists, Fontana stresses that popular identities are not a given of the social field; they must be constructed. His reading of Gramsci, however, specifies that process of construction as an educative logic of social relations implicated in the politics of party. The issue of whether the political order is a "single" space or, as in Laclau and Mouffe, a matter of intersecting, multiple contingencies is less important than whether the reciprocity of educational relationships can inform the activity of *every* citizen–participant. Fontana finds the basis for Gramsci's conception of educational reciprocity in the formula Machiavelli introduces in both the *Discourses* and *The Art of War*: "The new knowledge that Machiavelli intends to introduce is here established by the relation 'io . . . da voi . . . come voi da me' ('I . . . through you . . . as you through me'), a relation that establishes the active dynamism of a knowledge that itself teaches the necessity for action" (p. 104). For Fontana, education in this Gramscian sense is a precondition for general political freedom, rather than a political strategy articulated by a representative party leadership.

The largest claim of the book is that no matter how "post," the reduction of politics to strategies, organization, and interests in fact continues a liberal, Crocean tradition of avoiding "the presence of the 'mass' as a new factor in the power equation." It remains within the field of politics as domination. To some great extent, this overarching claim is asserted rather than argued, for Fontana gives scarcely enough attention to recent political discourses to ground it. Nevertheless, the insight and the awareness of both historical circumstances and philosophical positioning that Fontana brings to bear on Gramsci's reading of Machiavelli seems to me an indispensable corrective to an all-too-familiar "post" assumption that a party politics engaged in fundamental, global conflicts is merely a thing of the past.

University of Washington EVAN WATKINS

The Politics of Community: A Feminist Critique of the Liberal-Communitarian Debate. By Elizabeth Frazer and Nicola Lacey. Toronto: University of Toronto Press, 1993. 268p. $50.00 cloth, $19.95 paper.

Like many other feminists, Elizabeth Frazer and Nicola Lacey, both of Oxford University, are dismayed at the nearly complete domination of the "contested terrain" of political theory by theorists who pay no serious attention to what feminists see as thoroughly persuasive arguments. Clearly, the structures of gender fundamentally affect political and personal life and the societies in which such life occurs. When this is acknowledged, it profoundly alters the estimates that can be made of the adequacy of liberal political theory and communitarian social theory. Yet both the liberalism dominant in North America and its communitarian critics have done almost nothing beyond changing a few pronouns to take account of "the gendered reality behind the concepts of political theory" (p. 37).

Can anyone doubt that gender is a component of our identities, and that we are at least partly shaped by the structures of gender expectations and roles that underlie or frame or are woven into the social worlds we inhabit, whatever the metaphors we use to express this? But then the abstract, isolated individuals of liberal theory, and the view that political activity consists of the individual actions of separate entities, are conceptually deficient. And the traditional liberal distinction between public and private can be seen to be in need of radical revision.

On the other hand, can anyone doubt that women can resist the practices that structure gender in the communities they inhabit, or that women can construct alternative discourses in which to understand sexual harassment, or pornography, or the way the personal is political? And that women can sometimes succeed in making these issues political in the liberal sense? But then the view of the person and of agency offered by communitarian alternatives to liberalism are conceptually unsatisfactory. A view of social transformation employing political action that goes beyond the practices of any given community is needed.

In this discerning and important book, Frazer and Lacey are as interested in furthering the discussion taking place among feminists as in providing additional reasons to take feminism seriously for those who prefer not to. The authors suggest how feminist social and political theory can use the best of liberalism and the best of communitarianism in the development of our own theories. They understand that political theory without a theory of society and of persons cannot be satisfactory. They persuasively focus on the need to accept feminist and communitarian critiques of liberalism while resisting the communitarian tendency to see persons as determined by their social circumstances.

Against the liberalism of Rawls, Dworkin, Kymlicka, and Raz, they point out that "we experience ourselves as embodied and socially-situated being with multiple ties and commitments. Human life outside the context of human society, interaction and interdependence is evidently impossible. Therefore, the notion of the disembodied or pre-social individual is seen as an eminently unsuitable starting place for political theory, as is transcendence of our social being as a first political or ethical ideal" (p. 57). And they interpret liberal conceptions as historically and culturally specific rather than universal.

On the other hand, they counsel avoiding any alternative that would see the embodied and socially situated persons that political theory should start with as mere victims of circumstance, confined by their places in history and the traditions of their communities. Assessing the work of MacIntyre, Sandel, Taylor, and Unger, they worry that "the lack of critical foothold in communitarian analysis will tend to the rationalization of the status quo, whatever it happens to be" (p. 141).

Frazer and Lacey also argue that any satisfactory political theory must have as a central component a theory of power, and that this must enable us to understand the power of social structures, of discourses such as those illuminated by Foucault, and of groups, as well as of traditional liberal exercises of power by one individual over another. They fault both liberalism and communitarianism for lacking adequate theories of power. Our conceptions of power, they argue, must also include the power to empower, as in feminist practices of mothering and teaching, and, the authors argue, feminists should not shun power but use it appropriately to overcome domination of all kinds.

This book provides very useful overviews of liberalism, communitarianism, structuralism, deconstruction, discourse theory, and such concepts as practice, social structure, power, and self. Frazer and Lacey discuss knowledgeably various metaphysical and epistemological issues concerning, for instance, the reality of social structures, the status of groups, and the shortcomings of empiricism.

In a final chapter outlining their own recommendations, they put forward a "post-liberal, post-structuralist conception of political theory" (p. 190). It focuses on oppression and is committed to a political practice which gives "a central place to dialogue and the ongoing democratic involvement of members of the political community" (pp. 192–193). This approach has been called "dialogic communitarianism." Appreciative especially of the work of Seyla Benhabib, Drucilla Cornell, Nancy Fraser, Michael Freeden, Michael Walzer, and Iris Marion Young, Frazer and Lacey see power as pervasive in structures and practices, and the political as extending far beyond the state. Though they reject Richard Rorty's "radical post-modern pragmatism," they advocate embracing a diversity of interpretive, meaning-generating communities within broader societies; rather than fearing the fragmentations of postmodern life, they see in them increased possibilities for recreating communities and overcoming oppression. Arguing for a view of the self as relational, they stress the potential for such selves to engage in critical reflection and to transform practices. An important part of this involves culture: "in a real sense, to change discursive practice is to change the world" (p. 210). For instance, when unwanted sexual attentions are named sexual harassment and seen as part of a structure that disempowers women rather than as individual foibles, "they are no longer the same . . . The world is now one in which a practice which was formerly taken as a matter of uncontroversial entitlement has been constructed as problematic . . . What ensues is, in effect, a battle for dominant cultural meaning" (pp. 210–211). Contrary to liberal approaches, legal restrictions and political reform are not necessarily the most effective ways to achieve change. But contrary to communitarian tendencies, practices are not simply given features of culture; "culture is not monolithic."

One of the weaker sections of this impressive book is the authors' discussion of feminist revaluations of traditional female roles. For instance, Frazer and Lacey are suspicious of the ways in which arguments for the importance of caring for children, play into the hands of traditionalists wanting women to be satisfied with such roles, and they discuss childcare as an appropriate concern of government. We can all agree that feminists must constantly guard against the misuse of our arguments, yet at the same time insist that we not have our thinking about restructuring society paralyzed by such fears. Many feminists want the traditional roles radically restructured, as they would be if men participated fully in them. But many also argue that only a revaluation of such practices will make possible the kinds of social transformation we should seek. The activity of childcare, for instance, should be valued as highly as, say, corporate management; the profession of teaching children should be seen to be of as much importance as professions with many times more influence at present. In a feminist society, various traditionally female practices would not only be transformed in themselves, but the relations between such practices would be radically restructured. Instead of being, as now, continually overshadowed by economic and legal and political institutions, concern for the best possible development of children would be at the center of feminist society and would provide the point of view from which much of the rest of social life would be evaluated.

Frazer and Lacey tend to call any recommendations for improving social arrangements "utopian;" this seems a questionable characterization and a departure from their own perceptive understanding of culture and social change. Many proposals made by feminist theorists to reduce oppression call for what is urgently needed and practically possible.

City University of New York, Graduate School and Hunter College VIRGINIA HELD

Hannah Arendt: Politics, History, and Citizenship. By Phillip Hansen. Stanford: Stanford University Press, 1993. 266p. $39.50 cloth, $14.95 paper.

Following Claude Lefort, Phillip Hansen reads Hannah Arendt as a theorist of the *political.* As Hansen conceives it, the political signifies the constituting power and spirit of collective life, rather than politics in a narrow institutional sense. The central concepts of Arendt's political ontology—freedom, action, and the public realm—provide critical standards to grasp the decline of "genuine" politics and suggest the outlines of political association built upon plurality and "sheer human togetherness." Hansen finds Arendt an ideal conversational companion in his effort to judge the contemporary prospects of emancipatory politics.

"False" politics ("forms of social life which deny solidarity and citizenship" [p. 89]) arise when genuine forms of political thinking and action are blocked. Human (ontological) needs, Hansen avers, grant potency to inauthentic politics because when "the possibilities for a genuine politics are checkmated, human action will find other outlets" (p. 10). Hansen provides an excellent illumination of false politics, and this is a noteworthy achievement given the relative neglect of these phenomena in much of the most novel, recent writing about Arendt. His treatment of the crisis in culture, organized political lying, violence, and totalitarianism are consistently instructive and thought-provoking. His exposition of Arendt's affirmative political vision is less satisfying. This may be attributed to Hansen's interpretive strategy. Because he is reluctant to "[push] forward with the development of Arendt's ideas" (p. 12), he leaves us with problems which her thinking crystallized. From another perspective, however, Hansen's exegetical skill makes his a sophisticated introductory text for those less familiar with Arendt's writings.

The book's title captures Hansen's insight that for Arendt "history and politics inhere in one another" (p. 48), which is to say, our conception and experience of history condition the character of citizenship. False politics thrive when we experience history as a "logical" or "lawful" process "which both compels and validates individual and collective activity" (p. 7). Loss of "conscious responsibility, moral sensibility and, in extreme cases, the ability to think at all" (p. 7), is the price of such "process-thinking." Genuine politics not only consciously direct social processes (Hansen explores this by reading Arendt in dialogue with recent theories of human need); it also interrupts processes to begin something new. Totalitarianism is a logical consequence and extreme example of false politics; the promising moments of modern revolutions exemplify genuine political forms.

Tracing Arendt's persistent if vexed relationship to Marx distinguishes Hansen's account and reveals one possible reason for the explosion of interest in Arendt: her writings are a treasure trove for radical critiques of late modern societies that abjure many of Marx's untenable historical and philosophical assumptions. As Arendt teaches, "Marxism emphatically sustains rather than challenges those forces in bourgeois society that tighten the stranglehold of necessity and undermine freedom" (p. 182). The antidote is a recovery of the political.

Hansen takes Arendt's task as his own: to reconceive what it means to think and act politically. He traces the connections in Arendt's work between public life and political thinking. Most notably, he interprets "political thinking *as* politics" rather than simply "thinking *about* politics" (p. 197). Thinking, he claims, entails coming home to oneself while political action concerns trying to be at home with others. According to Hansen, Arendtian politics has a distinctly personal character. At issue is the quality of our relationship with ourselves and others. Our relations are genuinely political when we are simply with others, neither for nor against them. Though political thinking and action are definitive of "what it is good to be" (p. 193), they are also daunting and difficult. They are demanding because our lives are "neither inevitably fated nor radically undetermined" (p. 4). Openness in the face of the contingencies of this human condition, resisting the desire for sovereign mastery over the world and others, is the political challenge. We share this world with others who are both distinct and like ourselves. This is why political thinking and action are estimable: They foster and reveal our unique individuality while creating the possibility of solidarity. Indeed, who we are cannot be disclosed without human togetherness. Hansen rightly regards preserving plurality as the principle inspiring Arendt's thought-deeds. Whether endeavoring to feel at home with ourselves and in the world is the best (or Arendt's most suggestive) way of caring for plurality is another matter.

Can "genuine communal solidarity" exist without expunging individuality or violating others? Hansen answers affirmatively and invokes Arendt's public realm constituted by plurality. The commitment to plurality—as "identity in difference" (p. 49)—limits what you can reasonably do to others while at the same time securing a common world "where trust in what one's senses reveal is guaranteed" (p. 182). The skeptic might ask what is to prevent this feeling of being at home in the world from becoming a rote conformity to conventional morality which might serve evil as well as condemn it? Arendt's answer, Hansen maintains, is political thinking, especially political judgment which "involves both thinking in the stead of another and the capacity to determine 'right' and 'wrong' " (p. 209). Thinking fosters judgment, as Arendt interprets Socrates, by bringing us home to ourselves. Those who come home to the voice of conscience are more reticent to do wrong because they must live with themselves and, thus, wish to remain on friendly terms with their internal interlocutor. But if we conceive plurality as difference that precludes identity (and other aspects of Arendt's thinking suggest that we should, eg., the violations inherent in the activities of *homo faber*) then thinking politically may entail something more and different. Perhaps

Socrates resisted evil because he felt the otherness of his self. He never ceased to consider how his individual and Athenian home were predicated upon actual and potential harm to others even as (that much more so as) he showed himself to be Athens' most free and responsible citizen. Hansen's concluding reflections on Arendt's judgment of Heidegger and Jaspers suggest that he personally lives the dilemmas of plurality with the "dignity, decency and courage" (p. 198) Arendt esteems. But theoretically articulating the political conditions of doing so may require not only thinking with and against, but also beyond Arendt.

University of Illinois, Urbana-Champaign MELISSA A. ORLIE

The Economic Foundations of Government. By Randall G. Holcombe. New York: New York University Press, 1993. 273p. $50.00.

This is an interesting, if flawed, book, another of the genre of books that look at government through the lens of microeconomic theory. (The author is an economics professor.) Partly, it has the feel of a brief text, presenting yet again a public choice perspective on big questions such as the origins and nature of government. It has, however, a number of original and intriguing ideas as well.

Although Holcombe's book covers a range of topics, the basic ground it covers is relatively straightforward. Government arises as a trade—"protection for tribute." People pay governments money in exchange for physical and economic security. Once governments come into existence, however, they are able to act like the monopolies they are—selling protective services at monopoly prices and, more importantly, producing goods other than protection and selling them at monopoly prices, "in a manner similar to the way in which monopolists might try tie-in sales, all-or-nothing sales, price discrimination, and so forth to enhance their profits" (p. 9).

Holcombe's account of government as a trade of protection for tribute represents a significant addition to the literature analyzing the Hobbesian model. One question that has been raised recently in this literature is why the strong would have any interest in participating in a social contract: Wouldn't they be better off stealing from the weak? The point of this criticism is to suggest that one cannot justify the development of government strictly from a contractual perspective of agreement among self-interested parties without referring to substantive ethical concepts (e.g., the immorality of theft) to justify government as arising to enforce a moral vision going beyond an agreement among the self-interested. Hobbes had an answer for the criticism that the strong would be better off without a social contract, suggesting that even the weak might kill the strong. Holcombe has a more convincing answer. He argues that without some agreement setting clear bounds on the "tribute" that the weak must pay the strong, the weak will have no incentive to produce anything (because all of it is subject to theft), and the strong will be worse off than if they agree to limit the tribute they take, since they will get a larger proportion of a much smaller pie. (As an aside, Holcombe notes that a weakness of the libertarian suggestion that "taxation is theft" is that taxation is predictable, while theft is not, so that the productivity-depressing consequences of taxation are fewer.) In Holcombe's social contract, disparity in initial bargaining power affects the results of the contract: the strong come out better than the weak. But both sides are better off, and the trade occurs. Thus, no appeal to morality is necessary to produce government.

One quick (and I hope not unfair) reaction to Holcombe's clever argument is that if it took a bunch of smart social scientists until 1994 to develop this argument about why the strong will realize that it is in their interest to tax instead of steal, it is questionable whether the prehistoric strong would have all been so smart. There are certainly elements of Holcombe's argument that have a ring of plausibility. Indeed, they bear some resemblance to the emergence of feudalism in medieval Europe. However, the feudal lords who bestowed protection in exchange for tribute were, so to speak, protection specialists who were protecting the weak against potential threats from many who apparently did not realize that the weak would be more productive if only no efforts were made to steal from them. That does not get us to a social contract agreed to by the strong as well as the weak. If one is to justify this arrangement, there is still no way of escaping a moral judgment about the behavior of potential thieves.

Holcombe's argument about government as monopolist is also intriguing but likewise flawed. Government's monopoly over protective services creates the opportunity for government to extract monopoly profits. Although Holcombe never states this explicitly, his view (consistent with the ideology of most public choice scholars) seems to be that essentially everything government does beside providing protection (everything from farm subsidies to antipoverty programs) involves government extraction of monopoly profits from citizens through a tie-in deal whereby they are forced to support such programs as a condition for receiving protection from theft. If government can exploit its monopoly position, those who hold power may use it either to adopt special-interest legislation or to vote their ideological convictions about good public policy without worrying about electoral defeat (actions Holcombe appears to regard as essentially equivalent from his analytic perspective).

Government's ability to act as a monopolist, Holcombe argues, is restrained in the same ways that monopoly power is restrained in the private economy—through competition among units vying to be our government. This may take the form of Tieboutian competition among local governments, based on individual mobility, for attracting and holding residents. Or it may take the form of democratic electoral competition in any one government, keeping in mind Harold Demsetz's insight that in the course of competition for the right to be a monopolist, monopoly profits will be competed away.

As a result of his view that competitive forces are crucial to reduce government monopoly profits, Holcombe emphasizes "barriers to entry" that make it more difficult for these competitive forces to operate. For example, Holcombe argues that barriers making it easier for incumbents to be reelected and increase government monopoly profits. He also presents the view that the oft-noted expansion of the role of government in connection with wars results from the decline in internation population mobility in connection with wars, which increases government monopoly power by short-circuiting the Tieboutian mechanism.

Holcombe's whole analogy of non-protection-related government programs to monopoly profits is, however, incomplete. Partly, there are some technical problems with the analogy, since the putative monopolist (elected officials) would seem to be interested in gaining a monopoly (i.e., assured reelection) for its own sake or for reasons not relating to extracting monopoly profits from citizens (e.g., simply enjoying the power to make decisions), rather than, as with a normal monopolist, for the sake of its instrumentality in extracting monopoly profits from customers. More importantly, in Holcombe's view, government expansion arises only from the activities of special interest groups or through the personal ideological agendas of elected officials. This ignores the fact that electoral competition is itself a major reason government has taken on new roles. The majority, Holcombe suggests (though he never specifically states), would never go along with a government that provided more than protection. Hence, any such government must reflect monopoly profits. Holcombe's bizarre suggestion that government expansion in the wake of world wars reflected reduced international mobility ignores not only such obvious alternative explanations as the effects of war on national debt, postwar military spending, and benefits for soldiers but also the impact of wars on the climate of ideas in the postwar eras.

Now, from a libertarian moral perspective (including James Buchanan's view that government policies should be required to elicit unanimous support), the fact that government has taken on these roles may be morally unacceptable. Holcombe, however, seems to want to use a majoritarian framework to make what is probably in fact a libertarian, minority-rights point about the expansion of government. That will not work.

There is a final observation about this kind of economic analysis applied to government. On the one hand, the use of economic models can provide valuable insights and new ways of thinking (e.g., seeing incumbency advantages as analogous to economic barriers to entry that increase the ability to exact monopoly profits). On the other hand, the danger of reductionism is that when one phenomenon (politics) is reduced to another (economics), important features of the phenomenon being reduced can get lost. The basic metaphor of government as a marketlike arena for self-interested exchanges, where normative considerations play no role, is an example of the dangers of reductionism. Political scientists must resist efforts to reduce politics to economics.

Harvard University and STEVEN KELMAN
U.S. Office of Management and Budget

Paradigms and Barriers: How Habits of Mind Govern Scientific Beliefs. By Howard Margolis. Chicago: University of Chicago Press, 1993. 267p. $40.00 cloth, $15.95 paper.

Margolis presents *Paradigms and Barriers* as the second of three books on "persuasion and belief in the context of social choice" (p. xi). Scholars of politics are likely to find most intriguing the first and last quarters of the book, where Margolis explains how "habits of mind" help, hinder, and undergird reasoning across several sciences. (A middle chapter on "The Emergence of Probability" addresses a case of interest to political scientists; but the other chapters in the middle relate stories from early chemistry, astronomy, and physics.) Like the prequel, this book speaks to political scientists on four main levels.

Students of political cognition might find useful several features in the Margolis account of all cognition as recognition of patterns. An obvious limitation of this as an elemental treatment is the circularity in parsing cognition as re-cognition—as Margolis more or less admits. Nonetheless the Margolis approach can spur us to inventory recurrent patterns of perception as figures of conception that typify styles of thinking—whether in politics or elsewhere. Then we can examine their ties to one another and their environments. This is what Margolis does when specifying the "habits of mind" that sometimes aid and other times impede scientific inquiries. When persuasion is at issue, as it often is in politics, these habits of mind are the figures (or tropes) of experience and reasoning that inform elite references, public opinions, political myths, enemy interpretations, and media reports. The Margolis "habits of mind" are thus akin to the social conventions noted by Ludwig Wittgenstein, Stanley Cavell, and social-constructionist students of science. And cognitive scientists who trace strongly valenced paths of spreading activation in associative networks can plow the same ground, though in very different ways and with very different results. Margolis takes himself to sidestep specifically political patterns for most of the book at hand, yet that premise would hold only on a narrowly liberal notion of politics as activity related directly to the state.

Students of political choice, accordingly, can look to the book's case studies for reconstructions of rational choice in some of its more institutionalized and rigorous settings. The situational detail central to the Margolis analysis would be a wonderful antidote to the increasing abstraction of rational-choice work on politics. Where the retreat to formal theorems threatens to leave positive political theory as bereft of political substances as mathematical economics have become empty of matters economical, the historical ambitions of the Margolis approach can prove highly valuable. At times Margolis settles for the analytical philosopher's style of spinning a brief but plausible tale, without bothering to think through plausible counterarguments or stories from other perspectives. But these are lapses of execution, not method.

Students of political epistemology and methodology can gain from Margolis a sense of how social-constructionism can help us to understand political inquiries and practice them better. Margolis takes pains to rebut the arguments for epistemic relativism often advanced by social-constructionists. Indeed, he, like his targets, gives so much space to this old agenda of philosophical puzzles that the book degenerates on occasion into the idle kinds of dispute disparaged by cognitive science. Even so, the Margolis stories of science imply innumerable corrections to the objectivist canons of some singular Rationality or Method for studying politics. Showing habits of mind to differentiate paradigms and disciplines, Margolis quietly dispels idols of unified science. How much the author intends these lessons, I am not sure, but the book offers them readily to anyone who reads its case studies carefully.

Students of political persuasion can turn to the Margolis book for case studies and more. Margolis provides an anti-inflammatory sense of how politics, culture, and science entangle both constructively and not. He emphasizes how "to a striking extent what pass for good arguments are the same everywhere" (p. 199), and he downplays that importance of moves in rhetoric by saying that any decent rhetorician in science or politics will display "a mixed rhetorical style" that uses many devices (p. 197). Nevertheless his case studies often specify and always reveal intricate strategies and dynamics of persuasion of great interest in political terms. Margolis resists regarding persuasion as the politics of talk or reasoning, but his book helps us appreciate the politics amply involved in habits of mind important for episodes from the early modern sciences.

Gracefully written, the arguments of *Paradigms and Barriers* depend on a style of rational reconstruction and storytelling long common among philosophers of science. Margolis seems to acknowledge that his own rhetorics differ markedly from those now prominent in cognitive science or in accounts of rational choice. They have shortcomings, to be sure; still they comprise a voice that should be heard more in various studies of politics.

University of Iowa JOHN S. NELSON

Explaining Political Disagreement. By Andrew Mason. New York: Cambridge University Press, 1993. 170p. $44.95.

One of the central purposes generally ascribed to the liberal state, in both its minimal and robust incarnations, is dispute management. The metaphor of the forum expresses both the ambitions and limitations of this management. Persons introduce rival claims into a fixed space where they are to be adjudicated by public reason. The background conditions that produce the disputes are not addressed by the forum. The successes and failures of liberal dispute management famously occur within the parameters of deliberation among reasonable persons.

Andrew Mason provides a set of arguments, closely tied to relevant literatures, that attempt to explain political disagreements. Unfortunately the criteria for good or even adequate explanations are never specified in the work. One consequence is that it is never clear whether Mason is explaining the occurrence or persistence of disagreements. But this flaw aside, the work does advance our understandings of disagreements that might lead to political disputes. Mason even appropriates the signature mark of the liberal forum in distinguishing the reasoned approach of liberal governance from nonrational considerations. It is a mark of Mason's acumen that he attempts to combine these two species of explanation in a single model. The ultimate inadequacies of this synthesis do not subtract from the merit of the work.

Mason recognizes a distinction between two conceptions of disagreement: imperfection, in which at least one of the parties to a dispute is wrong, and contestability, where the proper use of political terms allows for a variety of reasonable interpretations of them. The first conception explains disagreements, and presumably resolves them, with a theory of error that accounts for mistakes. The second relies on the open-ended qualities of language for explanations.

Mason develops a hybrid argument that combines these two conceptions. He accepts the idea of moral truth and error, though he denies that moral or political disputes are resolvable through the correction of error. His version of moral cognitivism relies on norms of inquiry (found in both science and morality) rather than traditional moral realism. This Quinean approach allows him to accept essentially contested concepts and the persistence of reasonable disagreements, while allowing that some interpretations may be superior to others. Mason's view is a variant on the familiar coherence theory that is currently regarded with so much good will in political and legal philosophy.

At the end Mason provides a materialist explanation for a variety of disagreements that integrates the gender differences introduced by Gilligan with the psychoanalytic theory of gender construction developed by Chodorow. This model relies on the sexual division of labour to explain some political disagreements. Mason also uses the model to account for the attraction of Nozick's story of how persons acquire self-esteem.

No outline of the arguments can capture the intelligence of so many discussions in the book. Mason offers insights on the failures of explaining disagreements in terms of incompatible value premises, the difficulties in using distinctions between a concept and a conception, the desirability of regarding moral justification in terms of diverse forms rather than a single model (thus rejecting Hare's approach), the powers of analogical reasoning in moral discourse, the distinctions between ambiguous and essentially contested terms, the unintelligibility of the assertion that paradigms can be both incommensurate and contradictory (an assertion usually attributed to Kuhn), and much more. The reader will relish these sections even when dissenting from key observations and conclusions and wishing for sharper distinctions between disagreements and disputes.

Any reservations about this book are inspired by the intelligence of the contributions. My own thoughts are that the author fails in his stated goal to construct an empirical explanation that integrates rational (reasons for judgments) and non-rational (e.g., psychological propensities and social structures) considerations. It is true, as Mason points out, that "explanations which are capable of operating at each of these levels are potentially very powerful" (p. 15), but his ending theory does not achieve this power. The rational account of disagreement is followed in the text by the materialist theory. No model integrates the two.

The absence of an integrated theory may itself be explained by difficulties in separating rational and nonrational considerations rather than by Mason's failure to seize an opportunity. We might distinguish between the sources of disputes and their prolongation and even intractability. Persons can have conflicting preferences, interests, ideologies, and understandings of reality, any of which can lead to disputes that reach the political domain. The causes of social conflicts are multiple. The two material considerations cited by Mason—social structures and psychological factors—have received primary attention in social theory. But another *material* consideration must be discourse itself. Ideas, and their expressions, have an inertia that affects perceptions and understandings of experience over historical time, and perhaps structures realities. Discourses can conflict with one another, leading again to political disputes. Mason refuses to consider the causal relations between reasons and beliefs. It may be that rational considerations are also nonrational explanations, an integration unfortunately dependent on reductionism.

Why disagreements persist is not the same as why they occur. Material explanations for disputes are causal, and may be generalizable to some degree across political societies. But the prolongation of disputes, their tractable or intractable natures, may be tied to regimes and seem eminently contextual. An authoritarian state may admit no intractable disputes, resolving all political differences with effective force. A theistic state, governed by truth, may regard all disputes as occasioned by error on the part of some or all of the disputants. Corrective mechanisms follow the imposition of a theory of truth. In liberal regimes, however, simple force and dominating truth are not available options for the state in managing disputes. A language of public reason must prevail. The contestable view is that this language admits multiple reasonable interpretations, which leads to the intractability of political disputes. Contestability may not be a product simply of language, however, but of a political setting that resists closure. The contestability of language may be a function of its political contexts, at least in the sense that regimes can activate or suppress the elastic possibilities of terms. Is the relationship between language (on this view) and political disagreement a rational or a materialist explanation? The fact is that we do not have an adequate account of material and non-material considerations that will allow us to ponder how to integrate them. The liberal reluctance to explore background variables in adjudicating disputes may be based on a separation between the rational and the non-rational that needs to be abandoned rather than closed with an integrated model.

But these are the kinds of speculations that Mason's book inspires. The arguments developed by Mason are important contributions to an ongoing conversation about political disagreement. Adding to the agenda of distinctions and criteria will be a normal reaction from readers forced to think about these problems at a different level. No higher tribute can be paid to a book.

Syracuse University FRED M. FROHOCK

Tyranny and Political Culture in Ancient Greece. By James F. McGlew. Ithaca: Cornell University Press, 1993. 234p. $31.50.

Athens on Trial: The Antidemocratic Tradition in Western Thought. By Jennifer Tolbert Roberts. Princeton: Princeton University Press, 1994. 391p. $29.95.

Both books under review here explore the transformation of a particular historical political regime into an ideological myth. James F. McGlew, in the far more focused work, studies the place of the tyrannies that flourished during the Archaic Greece of the sixth century BC in the language and emerging self-conceptions of the democratic city-states (or poleis) of the fifth and fourth centuries. Jennifer Tolbert Roberts, in a much more wide-ranging (and much longer) work studies the place of Athens as a democratic regime in the thought of a wide assortment of authors across the last two and a half millennia, up to and including the latest conferences and forthcoming books in twentieth century America.

Both authors confront a difficult historical challenge: They take what is in the first place a not fully understood political system and explore how others have used their own readings of that system to express views about their own political worlds and to influence the actors in those worlds. McGlew has the somewhat harder task in that he tries to study how the "city constructs and remembers its own political identity" (p. 10) from the stories and images of tyrants and founders; or, as he phrases it later in the volume: "The polis defined itself as a subject as it remade the world into its objective field" (p. 150). McGlew does not address the issue of agency, how it is that *poleis* or cities can act and, particularly, how they can re-write their own narratives. While his claims along these lines are somewhat discomforting, McGlew does force us to consider the interesting theoretical problem of the relationship between tyranny and democracy. We may not exactly learn how "images of sovereignty were the great political legacy of tyranny" (p. 9), but we are made aware that the tyrant—too often dismissed as a ruthless aberration in a political world that talks rather of authoritarianism and totalitarianism—should hold an important place in our conceptual framework as we try to understand the democratic regime.

Roberts has a somewhat easier time for she does not make causal claims about cities as actors and self-narrators; she instead writes a detailed intellectual history which recounts how Athens was perceived and interpreted in different ages and by different cultures. The accuracy or inaccuracy of the interpretations and the institutional effects of such readings is of less interest than the multitude of reactions to Athenian democracy that she finds in the seemingly myriad texts about which she writes. The texts she discusses reveal far more about the times in which they were written and what it was about democracy that found or did not find favor than they do about the practice of democracy, ancient or modern.

In Book Eight of the *Republic* Socrates offers a schematized portrait of the transition from democracy to tyranny, claiming that "it seems likely that from no other political regime than democracy is tyranny established" (564a). It is the ultimate freedom of democracy that leads to the slavery of the tyrannical regime in the a-historical analysis presented in Cephalus's house. McGlew, in contrast to Socrates, tries to analyze the transition between regimes historically, i.e., how tyranny gave birth to democracy, and show how some of the fundamental aspects of tyranny were translated into the life of the democratic city. In almost Hegelian fashion, we see how the democracies of the fifth century incorporate elements of the sixth century tyrannies that preceded them, especially in terms of freedom (*eleutheria*) and the autonomy of law. A chapter devoted to Solon explores Solon's efforts to remove the individual from the enforcement of justice, leaving the laws with their own power to provide order and thereby bring justice into the political community (p. 110). That Solon's attempt to establish the autonomous law failed, opening the way for the tyranny of Peisistratus, is for McGlew the result of Solon's reliance on "history" rather than "memory," or because "Solon's story was a product of his self-representation, not of popular legend" (p. 121). Nevertheless, Solon's story in McGlew's account suggests that one must consider the inherent relationship between tyranny and the rule of law, not as opposites, but as expressions of autonomy.

Autonomy emerges as well in McGlew's analysis of citizenship, perhaps more effectively than in the relationship between law and tyranny, for he draws together the freedom of the tyrant and the citizen: "The tyrant's power passed to his city not as a political abstraction or formula but as an individual possession" (p. 187). In a rather idiosyncratic reading of Pericles' funeral oration, McGlew tries to show the Athenian citizen as individual tyrant such that *erastes* from that speech, the lovers of Athens whom Pericles urges his listeners to become, expresses the "passion of the aspiring tyrant, not the loyalty of the honest citizen" (p. 188). This reading may raise questions, but it does alert us to the possibility of thinking about the tyrannical drives of the democratic citizen.

While McGlew tries to identify how the somewhat amorphous "memories" of tyranny and foundations become part of the political structures of succeeding centuries, Roberts traces more directly the "spectrum of responses to classical Athens" (p. 96). Vividly and deftly, she follows those responses through the millennia, blaming Plutarch most strongly for establishing the anti-democratic tradition that marred Athens's reputation. Though the hostile attitudes to Athens paralleled the anti-democratic attitudes of the numerous authors discussed before the eighteenth century, Roberts points out that it was not always political views that determined the response to Athens. In particular, the German author Wilhelm Winckelmann longs for the aesthetics of the Athenians; the resurrection of Athenian democracy in the eyes of this writer and other eighteenth century German authors occurs not from interest in the political regime that gives autonomy and freedom to the individual, but from the culture that Athenian democracy could produce. Similarly, in England in the nineteenth century, enchantment with the civilization that the Athenian regime nurtured justified its praise. And then—as Roberts nicely points out—the unity of democracy and empire could serve, by analogy, the contemporary political interests of a colonizing Britain. In a similarly intriguing way, Roberts shows how democratic Athens became part of the debate between abolitionists and the defenders of slavery prior to the American Civil War.

Roberts offers an enormous amount of information and discusses a hefty number of authors, often more than one per page, especially as we get to the last decades of the twentieth century. Through it all, Roberts reminds her readers that just as her subjects react subjectively to the Athens they are portraying, she too reads their works subjectively leaving us free to reread and reinterpret the works she discusses. As the book nears its end, it takes on more and more the feel of an extensive review essay where all articles touching on Athens in the last decade must be included; this appears to be part of her attempt to assert her own subjective place within the tradition of scholars writing about Athens, rather than present herself as an objective scholar assessing the quality and significance of what she discusses.

Neither book is particularly satisfying in the readings offered of ancient texts. Roberts, eager to show the antidemocratic tradition emerging from Athens, itself offers somewhat glib restatements of Thucydides (e.g., she summarizes Pericles' funeral oration in nine points), Plato, and Aristotle. That the *Republic* makes clear that only in a democracy could a Socrates emerge, that the *Meno* points to the reasoning abilities of a slave, that Socrates's supposed disdain for the many does not mean his refusal to try to educate all who will talk with him, all these and many more points are ignored in making Plato a classic (and classist) antidemocrat. That Aristotle's definition of a citizen is one possible only for a democracy is likewise ignored, as are his arguments concerning the superiority of collective wisdom in things political; the ambiguity surrounding Aristotle's discussion of the hierarchy of the master-slave relationship is occasionally recounted, but its significance is not explored. McGlew concludes his book with brief readings of the *Oresteia, Oedipus,* and the *Republic* to suggest how "Athens made tyranny safe for democracy" (p. 190), but the brevity of the analyses allows only for a pointing to conjunctions of terms associated with tyranny rather than a full exploration of the theoretical significance of those conjunctions.

To ask for more careful and sustained readings is perhaps unfair, since the two authors do not necessarily build their books around these interpretations; rather, they move us to reflect on how historical political institutions take on conceptual lives of their own across the centuries and how we as scholars may most appropriately study and assess the significance of these conceptual lives.

University of Michigan ARLENE W. SAXONHOUSE

Against the Market: Political Economy, Market Socialism and the Marxist Critique. By David McNally. New York: Verso, 1993. 262p. $17.95 paper.

Against the Market is an ambitious defense of an unpopular position. The crippling defects of Stalinist command economies have led many socialists to advocate versions of (democratic) market socialism, combining collective ownership with commodity production. McNally argues that this is an unwarranted concession to the false view that "society cannot function except through alienated institutions which evade popular control" (p. 3). Worse, it is a contradiction in terms. The choice is socialism *or* the market (p. 2). A challenge to the current hegemony of promarket ideas can only be healthy; but while *Against the Market* deserves a wide readership among those interested in the history of political economy, it does not make the case McNally professes.

He proceeds by way of dazzling survey of the history of markets and theories of their operation, both conservative and radical, mainly in England in the century from Smith through Marx. His erudition is astonishing, and he has an enviable gift for concise exposition of complex ideas and an illuminating way of locating them in their political context. His chapter on Smith as a moral philosopher as well as a political economist helps recover that thinker from his reputation as a prophet of greed. Likewise fine are his discussions of lesser-known radical thinkers, many of them working-class activists, who attached the mean Malthusian dogmas of laissez faire and its cruel operation, in part with Smithian tools that they turned to increasingly socialist purposes. His treatments of Owen, Proudhon, and their market socialist disciples should be read by anyone interested in the intellectual background of Marx's economics. Similar consideration of French socialism (Saint-Simon, Fourier) would have been nice.

In discussing Marx's critique of market socialism, however, McNally tends to paraphrase, rather than analyze. He criticizes Diane Elson's account of commodity fetishism (p. 214), but his own discussion of this difficult idea merely quotes and restates Marx's Delphic deliverances (p. 162). He endorses Marx's labor theory of value as if it were unproblematic; but it is generally dismissed by economists, including many Marxists. McNally faults the earlier historical figures for not attaining Marx's insights without showing that these are defensible. Owen and others may have erred in regarding exploitation as due to monopoly advantages—something arising in exchange—rather than, as Marx thought, in production. But even if Marx was right (as I agree he was), this cannot be assumed.

The book's main virtues as socially informed intellectual history are a clue to its weakness with regard to McNally's main thesis. He expounds the market debate up to Marx's time very well and explains clearly why Marx rejected the market socialisms of his day. But even if Marx's objections are good, it is not clear why we should reject those of *our* day. First, as McNally shows, Marx's main direct criticisms of Proudhon and the Owenists turn on a special feature of their views—that remuneration should be in proportion to labor contribution and exchange should be in terms of equivalent labor values. Marx argued, plausibly, that this could not hold in a competitive market. But contemporary market socialists (e.g., John Roemer; David Schweickart) do not favor this feature. McNally does not engage with the details any of these new market socialisms to argue that their specific features would be, as he claims, tantamount to capitalism.

Instead he argues, boldly but implausibly, that the real evils of capitalism (exploitation, alienated labor, and crises) are due to markets *alone* and not at all to private property or wage labor as usually understood. He rightly places the labor market at the center of the socialist project, arguing that the commodification of labor power is the main problem with capitalism (p. 5). But he maintains that "wage labor" can exist even if productive assets are collectively owed and there are no capitalists. As long as "the fund for wages [is] determined by market prices for . . . commodities," self-managed worker cooperatives will act as "their own capitalist[s]" (p. 181), be compelled by blind market forces to accumulate endlessly, and in general reproduce the defects (for workers) of capitalist firms.

This is quite misleading. The market under consideration does not have exploitation in Marx's sense (i.e., wrongful expropriation of surplus by nonproducers), since all are workers. Nor need this market be subject to crises if, as in some models, investment is planned by the state, rather than dependent on the animal spirits of private investors and full employment is guaranteed by law. Finally, far from involving endless accumulation, many economists think a main *problem* for self-managed market socialism is its tendency towards very low growth because firms resist expansion when that means dividing up profits among more workers. McNally is right that even in market socialism, market forces rule workers' lives in a way not subject to democratic control. That is a serious objection. But it is not tantamount to capitalism or to wage labor.

If "the anarchy of the market" and its lack of democratic accountability is the problem, what is the alternative? Any challenge to markets must specify in plausible detail how a nonmarket economy can avoid the catastrophic defects of Stalinist planning and go "beyond alienated labor, exploitation, competition, and crisis" (p. 223). The lack of such an articulated model is the second main problem with McNally's approach.

The central question is whether planning can offer the kind of control over our lives that markets deny. This, rather than efficiency, is the real bite to the Mises–Hayek "calculation problem," at least for socialists. In a modern economy, the amount of information about consumer preferences, producer capabilities, and alternative ways of matching these up is too vast for anyone to grasp or manipulate. If, *per impossible*, the information could be aggregated, imposing coordination on production units would mean that the plan would rule worker's lives. A democratic vote—or even democratic participation in planning—would translate into very little effective control. If the information is disaggregated and most decisions are left to production units, as McNally advocates, is the planning any more extensive than in market socialism or even modern capitalism? (He acknowledges that his approach is close to that of market socialists like Elson [p. 213].)

McNally claims that Mises and Hayek are wrong that markets are much better at processing information (p. 198). This is not to the point if *plans* do not enhance democratic control over our lives more than markets can. The case against markets requires that plans be better. In 30 quick pages, McNally sketches a "political economy of the working class" (pp. 184–212). But to vindicate Marx's communism, McNally would need a systematic treatment of the economics of planning, not an intellectual and social history of market socialism in the eighteenth and nineteenth century. Perhaps it is unfair to criticize an author for not writing a different book. To have made his case, that is what McNally would have to have done; but his history is exemplary and will be a lasting contribution.

University of Michigan Law School JUSTIN SCHWARTZ

Kant's Theory of Justice. By Allen D. Rosen. Ithaca: Cornell University Press, 1993. 237p. $28.50.

This is a clear minded and careful study of Kantian principles of justice. Beginning with the relation between justice and freedom, the author spells out, in Chapters One and Two, both the peculiar character of external freedom for Kant and the essential arguments by which (in the author's view) such freedom for Kant is justified. Chapter Three maps a variety of distinctions in Kant's ethical thought (e.g., between perfect and imperfect duties, and between those of wide and narrow scope); the result is a helpfully precise delimitation of the boundaries that both relate and separate justice and ethics for Kant (an area that has proved notoriously troublesome to scholars.) Chapters Four and Five turn to two fields of application especially contentions both in the scholarship and beyond: revolution and social welfare. The book concludes with a brief but judicious weighing of the strengths and weaknesses of Kant's contribution to liberal thought, and makes a forceful case for the superiority of Kant's categorical grounding of rights, over against the relativism and skepticism of other recent champions of liberal freedom. Throughout, Rosen's argument is enriched by examples from legal theory and case law (the dust jacket announces that

Rosen is a practicing attorney), examples that fruitfully extend the author's otherwise pertinent but narrow range of scholarly reference.

Readers, especially those who are not professional Kant scholars, will find particularly interesting Rosen's reconstruction of Kant's moral grounding of justice, and Rosen's effort to save Kant from some of the less attractive doctrines with which he has historically been associated: to wit, his denial of the right to revolution and his alleged endorsement of the neutral or "night watchman" state which has figured prominently in recent discussion within the discipline of political science and elsewhere. His treatment of the categorical imperative, which lays special weight on the relation between the form and content of the law, presents Kant's case with a precision lacking in many more elaborate discussions. By the same token, however, Rosen chooses to ignore the many difficulties and objections that figure prominently in the literature—difficulties beyond the well-known charge of empty formalism that Rosen ably dispatches. Moreover, Rosen's overriding emphasis on the technicalities of Kant's argument leaves little room for a broader consideration of the significance, and, arguably, the limits, of a Kantian orientation to questions of morality and justice over and against competing orientations. Why Kant's appeal to categorical demands is preferable, say, to the approach of Aristotle is never indicated. Nevertheless, readers who wish to have Kant's categorical grounding of justice laid out for them would be well advised to read Rosen's first two chapters with care. Rosen also spells out very clearly the nature of republicanism for Kant, with an especially helpful discussion of the relation between political and civic freedom.

Rosen's treatment of Kant on revolution is crucial to the author's general project of sympathetic reconstruction. For surely no doctrine is less palatable to modern ears than Kant's insistence that revolution, though in some extreme circumstances excusable, is never justified. Rosen agrees with Kant that active revolution (as distinguished from the passive disobedience that Kant under certain conditions allows) can never be *legally* justified. To rescue the right to rebel against unjust governments Rosen expands Kant's theory of justice beyond the boundaries of an "ideal theory of law" to which Kant himself, according to the author, limits it. The resulting sacrifice of textual consistency yields a moral, though not strictly legal, right of rebellion—a move with evident attractions, however they may ultimately conflict with other aspects of Kant's enterprise.

The author's discussion of Kant on social welfare has more immediate practical weight, especially given the recent influence of Rawls. Rosen takes as his point of departure certain textual ambiguities that bear on the state's authority to aid the poor. Rosen convincingly shows that, first impressions aside, Kant is not the early advocate of pure laissez-faire that he is sometimes taken to be, in as much as the state has for Kant cultural and moral, as well narrowly juridical, missions. This reader was less convinced by the author's attempt to deduce a political duty of benevolence towards the poor, especially given the difficulty, on his account, of arriving at a principled demarcation of the limits to such benevolence. Rosen here follows a long line of thinkers, beginning with Fichte, who have attempted to blunt the rigor of Kant's distinction between formal and material equality. While it is clear that the Kantian state is *authorized* (Kant doesn't say "required") to provide with the bare necessities of life those who would otherwise perish, it is less obvious how Rosen's more expansive reading can be reconciled with the basic principle of justice, which calls for the formal maximization of individual freedom. Whatever conclusions they ultimately draw, readers should find Rosen's discussions of these matters exceptionally stimulating.

One wonders, finally, if the author does not take an unduly narrow view of the significance of Kant's views of justice for his thought as a whole. Telling in this regard is Rosen's deliberate disregard of international law as unnecessary to his purpose; for Kant's cosmopolitan idea is arguably (as Rosen partially acknowledges in his treatment of the historical mission of the state) the driving aim of Kant's philosophic project. Still, this is a consistently illuminating and, in terms of its own aims, successful study of a topic long neglected, especially in the English-speaking literature. For the care and intelligence with which he addresses it, the author is owed a special debt of gratitude.

Boston College SUSAN MELD SHELL

AMERICAN POLITICS

Advertising and a Democratic Press. By C. Edwin Baker. Princeton: Princeton University Press, 1994. 203p. $24.95.

Air Wars: Television Advertising in Election Campaigns, 1952–1992. By Darrell M. West. Washington: CQ Press, 1993. 223p. $18.95 paper.

Since the advent of televised campaign advertising, the "marketing" of political candidates has been compared to the marketing of consumer products. As Darrell West reminds us, however, "In reality . . . political commercials have little in common with product ads" (p. 17). The former are more novel events, are more likely to be recalled by viewers, have a more limited time span with a fixed endpoint within which to operate, and are designed to capture a plurality (in primaries) or a majority (in most general elections) of the "market share." And, as these two books ably demonstrate, the political consequences of product and candidate advertising are different as well.

In *Advertising and a Democratic Press,* C. Edwin Baker argues that "advertising seriously distorts and diminishes the mass media's contribution to a free and democratic society" (p. ix). Baker's central concern is the impact of the media's financial dependence on advertising on the substance and distribution of their *non*advertising content. Beginning with a hypothetical (and innovative) economic analysis, he demonstrates that a shift from a "world without advertising to a world with advertising" (p. 13) could theoretically eliminate competitive newspapers, reduce diversity, inhibit the creation of new newspapers, lead to a decline in total newspaper readership, and reduce the total resources spent on editorial content. He then goes on to show (through an examination of aggregate historical trends and individual case studies) that as newspapers become dependent on advertising, the actual patterns of readership and competition closely match his hypothetical scenarios.

At the same time that advertising reduces competition and readership, it also, according to Baker, shapes the specific content of the media by encouraging news reports and editorials that are supportive of advertisers' interests, by creating a consumer mentality that makes readers more responsive to advertising, by reducing partisan or controversial reporting that might offend advertisers' potential customers, and by favoring the interests of middle-to-higher income audiences. Baker supports these conclusions with specific, if sometimes circumstantial, examples of both the reactions of advertisers to news content that violates these four tendencies, and of self-monitoring by news organizations themselves.

Having made his case for the deleterious effects of advertising, Baker turns to a proposal for remedying this state of affairs. Acknowledging both that advertising provides a number of benefits to consumers and news organizations, and that any attempt to ban advertising is unrealistic, he recommends a tax on advertising, the revenues from which would be used to subsidize circulation, thus making newspapers (and other media) somewhat less dependent on advertisers for revenue and somewhat more dependent on readers. This, he argues, would change the incentive structure of the media in ways that would increase competition, the diversity of content, and the number of readers and viewers. Baker highlights the benefits of his proposal and argues for its feasibility by comparing it to a number of alternative policies that employ either advertising taxes or audience subsidies. He concludes his book with an exploration of legal issues, arguing that little in current First Amendment doctrine would necessarily challenge the constitutionality of his proposal.

Readers familiar with the work of Ben Bagdikian will note a great similarity with Baker's argument, a debt he readily acknowledges. This similarity extends to occasionally drawing sweeping conclusions about the insidious effects of advertising from specific, sometimes only partially convincing, examples and cases. The real contribution of Baker's work is fourfold. First is the careful, systematic presentation of the underlying logic of Bagdikian's argument. Second is the use of economic analyses to show how an apparent benefit of advertising—the subsidizing of the cost of information to the public—can lead, ironically, to a less free and democratic press. Third is the variety of logical and empirical evidence he brings to bare in demonstrating these effects. And fourth is the specificity of his proposed solution to this problem. In the end, readers may not be convinced by his argument, evidence, or policy recommendation, but they certainly will be stimulated by them.

While Baker explores the long-term, systemic, and indirect effects of product advertising on democratic politics, Darrell West focuses on the immediate, individual, and direct effects of campaign advertising. Drawing on a random sample of 150 "typical" presidential campaign spots aired between 1972 and 1992, and a purposive sample of 324 "prominent" spots aired between 1952 and 1992, he begins with a detailed analysis of the content of televised campaign advertisements. His findings both confirm and challenge conventional wisdom regarding this often criticized form of political communication. Among his more important conclusions are that campaign spots provide fewer mentions about public policy than prior research suggests; that only about half of these policy mentions provide "clear statements of past positions or expectations about future actions" (p. 36); that Democrats are more likely to emphasize domestic policy while Republicans are more likely to emphasize foreign policy in their television spots; and that attention to the personal qualities of candidates is somewhat more common in nominating than in general elections. West also uncovers several illuminating trends in the content of campaign spots over time. Most notably, he finds that since 1980 attention to specific policies has increased in prominent ads, while attention to candidates' personal qualities has decreased; that after 1956 specific appeals to party loyalty have been almost non-existent; and that negative advertising has generally increased over time (though it did decline in 1992). Within these general patterns, West also notes that the substance of political advertising varies in more election-specific and intuitively understandable ways; for example, mention of social welfare issues was most prominent in 1964, taxes in 1984, issues of war and peace in 1968, and so on.

Scholars such as Kathleen Hall Jamieson suggest that the news media have recently begun to cover political advertisements as campaign stories in their own right. West provides compelling evidence that this is indeed the case, with coverage of campaign ads by the *New York Times*, the *Washington Post*, and the *CBS Evening News* increasing substantially in the 1980s and 1990s. Especially revealing is his finding that two-thirds of the content of new stories about campaign advertisements are devoted to "horse race" issues (for example, how a particular spot fits into the candidate's larger electoral strategy). This emphasis on the strategic, as opposed to the substantive, import of campaign ads has been especially prominent since the early 1970s, though as West notes, in 1992 both the print and electronic media devoted considerable space to "Ad Watches"—stories which help citizens evaluate the accuracy and intended impact of specific campaign spots.

Having provided a thorough overview of the content of campaign commercials, West devotes the second half of his book to an examination of the impact of campaign spots on what voters learn about candidates, on what they see as the most important issues of the campaign, and on the standards they use for assessing the candidates. Using a variety of national and state public opinion surveys, he finds that citizens who were more exposed to political advertising (based on a four-point scale of reported attentiveness to political advertisements) were more likely to recognize the candidates, to rate them favorably, and to see them as potentially electable. Exposure to campaign advertising also affected voters' assessments of the personal traits and issue stands of candidates in ways consistent with the strategic appeals of the campaign ads themselves. Throughout his analyses, West controls for party identification, education, age, sex, race, ideology, political interest, and general media exposure, increasing one's confidence that the reported effects are due specifically to exposure to campaign advertising.

West also finds that campaign advertisements can be influential in setting the political agenda of voters and in establishing the particular issues used in assessing candidates, again controlling for possible confounding effects. Those who were most heavily exposed to ads often differed from those who were less heavily exposed in their views of the most important problem facing the nation and the most important event of the campaign. Significantly, these differences were consistent with the messages contained within the political advertisements themselves. For example, exposure to George Bush's 1988 "Revolving Door" spot (which criticized the Massachusetts prison furlough program that was in place during Michael Dukakis's tenure as Governor) increased the likelihood that voters believed crime was the most important problem facing the nation. Campaign ads were also effective in increasing voters' perceptions that a candidate was "doing well" in the campaign. Similarly, West finds that exposure to campaign ads can lead voters to emphasize certain issues in their evaluation of candidates (priming) while deemphasizing others (defusing). For example, in 1988 George Bush was able to defuse the potentially harmful issues of his record on the environment and his purported lack of compassion for people: Those most heavily exposed to campaign ads were least likely to report that these issues were important to their vote. And in 1992 the Clinton campaign appears to have successfully primed voters to focus on the nation's poor economy, while also increasing their perception that Clinton was best able to address this issue.

While making the *general* argument that campaign advertisements have a significant impact on political learning, agenda-setting, priming, and defusing, West is also careful to note (and show) that the effectiveness of a candidate's media campaign ultimately depends on the effectiveness of the opponents' ads, the timing of particular advertisements, the way in which the news media interprets the campaign, and the larger social and political context in which a particular election takes place. Effects can vary among groups of voters as well—for example Bush's "Revolving Door" spot, which specifically mentions rape, was particularly effective at increasing concern about crime among women. In addition, the impact of campaign advertisements is generally stronger in campaigns or for candidates with lower initial visibility. In the end, West concludes that while "television could become an enlightening force in American elections" (p. 157), the current content of campaign advertisements, coupled with the tendency of both the news media and candidate organizations to think about ads in strategic rather than educational terms, has led to an information environment that is much less useful to voters than it might be. He also raises the concern that, absent appropriate watchdog institutions, campaign advertising can manipulate, rather than educate voters. His main solution to this problem is for journalists to take their oversight function more seriously, increasing and expanding the kinds of "Ad Watch" stories they turned to in 1992.

Air Wars is an important contribution to the literature on media and politics for several reasons. The content analysis of campaign commercials is the most complete and thorough I have seen, and should serve as a valuable resource for students of campaigns and elections (indeed, I wish West had devoted more of his book to this part of his research, and tied it more directly to his subsequent analyses of the impact of campaign ads). And while learning, agenda-setting, priming, and defusing have been well-documented by experimental studies that focus on the news media, West shows that these effects can also be found using the somewhat more natural design of voter surveys conducted during actual campaigns. In addition, he demonstrates that these effects can result specifically from exposure to campaign advertising. Finally, by examining a number of different elections, and by focusing on both general exposure to advertisements and exposure to specific ads, West is able to identify both the systematic and case specific impact of political advertising, and to point to the factors that are likely to make ads effective (though more attention to how campaign advertising affects different subgroups within the population would have been valuable). Of course, the fact that the impact of campaign advertising often varies from election to election could be seen as evidence of the relatively weak, idiosyncratic effects of such ads, rather than as evidence of the complex interrelations West often posits. Nonetheless, in the end the empirical evidence and reasonable assumptions presented in *Air Wars* makes a compelling case for the power (and limitations) of campaign advertising in American elections.

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The Constitution of Judicial Power. By Sotirios A. Barber. Baltimore: Johns Hopkins University Press, 1993. 279p. $25.95.

In this work, Sotirios Barber sets forth a theory of judicial activism, in the tradition of the Warren Court, which he defends against "skillful enemies" on the right and "incompetent friends" on the left. Justices should make up their own minds as to what the Constitution means, giving no weight to the opinions of the framers, prior courts, legislators, current or past majorities. This practice, Barber maintains, is not the innovation of the Warren Court or the deviation of the Old Court, but in fact the "classical" theory of judicial review. In supporting this point, he closely analyzes the *Federalist Papers*, not exactly to reconstruct the arguments as Madison and Hamilton understood them, but to construct an interpretation that best combines plausibility with morality. In this way, Barber places the role of the courts in the context of *The Federalist*'s aspiration to reconcile democracy with justice, not "justice" but real justice. To simplify, and thus to do a real injustice to a complex, imaginative, and well structured argument, the Constitution assumes a gap between justice and what the people believe about justice, between the people's aspirations for simple justice and their immediate and often self-centered interests, and it "institutionalizes a concern" for narrowing these gaps by creating structures of government that can be responsible to the peoples' aspirations exactly because they are not responsive to their immediate interests (pp. 23 and 31). Active use of judicial review in the name of simple justice is the peak institutionalization of this concern.

In dwelling on the arguments of the framers, of course, Barber wishes not to bolster a jurisprudence of original meaning, but to undercut it. The framers, by his analysis, wanted the Court to pursue real justice, not just somebody's opinion about it, including their own. Barred by his own methodology then from resting this case for judicial activism on the intent of the framers, Barber furthers it by drawing on the revitalization of moral realism in philosophy, especially in the work of Michael Moore, to defend the presuppositions underlying it: that the Constitution refers to a moral reality distinct from anyone's conception of it, that judges are capable of being guided by this reality rather than partisanship and self-interest, that the American people are united as one body from the founding in their aspiration to know and realize justice as the central element of moral reality (from which aspiration in fact derives their authority), that it is respectable to think that such moral reality exists to which the people aspire (even if we cannot know it with certainty), and that the Constitution actually embodies such commitment to simple justice.

With this classical case and its presuppositions, Barber defends judicial activism from skillful enemies and incompetent friends. He is most effective in exposing the incompetence of its friends, who in simple-minded or highly sophisticated ways end up defending judicial review as an instrument of popular willfulness. This is true even of those who share Barber's vocabulary of constitutional "aspirations," such as Michael Perry, or the important distinction between concepts and conceptions, such as Ronald Dworkin. In one way or another, because they refuse to take seriously the possibility of real justice, these theorists end up rationalizing the conventional.

Barber is also concerned to identify the skill of judicial activism's enemies. That skill, he argues, lies not in the merits of their arguments. Indeed, the typical enemy, such as Robert Bork, cannot truly defend himself because his skepticism reduces all principles, which must include his own, to mere "preferences." Instead, their skill lies in appropriating as their own *The Federalist Papers*, John Marshall, Joseph Story, and James Bradley Thayer. As Barber attempts to show, not always persuasively in my mind, these thinkers support his classical theory of activism. The atypical enemy of judicial activism, whom Barber cannot so easily dismiss, is Walter Berns, who, like Barber, reaches to nature rather than convention for his argument. Yet in his neo-Hobbesian exposition of the Constitution, Berns cannot escape the paradox that plagued Hobbes as well—how to defend philosophically a regime that seeks to silence philosophic disputation as it liberates human acquisitiveness.

Barber is a platonist, his approach socratic. In masterly readings of commentator after commentator, fundamental premises are laid bare and shown to be at best paradoxical. Positivists are shown to require the normative philosophy they despise. Pragmatists are shown to presuppose the philosophic realism they would rather set aside. Especially in making the case for philosophy and, more particularly, moral realism, in constitutional interpretation, Barber is powerful. His book is a landmark achievement, one from which future arguments will have to take their bearings. That is not to say his argument is thoroughly sound—I, at least, find the step from moral realism to judicial activism a large non sequitur—but that it is plausible and thoroughly provocative. Whether one agrees or disagrees with Barber, one will find it hard to resist writing in the margins, or elsewhere, a companion volume of queries and comments. Here I will mention only a couple of concerns.

In emulating Socrates, Barber has set for himself an impossibly high standard, but since it is of his choosing, to measure him accordingly seems fitting, if not quite fair. In tone, his argument falls short of that steady detachment characteristic of the philosophic perspective. Towards the left, he in fact is calm and unstinting in his scholarly courtesy. Even as he points out the nihilistic implications of their thought, he acknowledges the possibility that they may be right. Towards the right, on the other hand, he is, well, excited. He repeatedly accuses those he calls "New Right" jurists (that is, the advocates of original meaning) of conducting a "McCarthyite" campaign against constitutional theory—also termed a "jihad" and a "crusade" (pp. xii, 119, 146, and 198; 128 and 149–50). Although he would not, of course, wish to call into question their underlying motives or raise concerns about the company they keep, he hints of something dark stirred into their attack on judicial activism: "Partly because academics tend to avoid open statements of political preference—few New Right scholars admit to racism, religious zealotry, greed, a scapegoating resentment of selected forms of welfare, and a desire for vengeance against selected classes of criminals—they make institutional arguments instead" (p. 1). One can almost hear the book announcing, "I have here, between my covers, the names of several originalists now teaching within the academy." The tone is especially unfortunate, for originalists, who have much to learn from his arguments about moral realism, will be able to listen only with heroic forbearance.

He also seems not to get the joke of Plato's *Republic.* Instead of seeing Socrates's constitution as an ironic impossibility, a perspective from which we might view the human comedy and endure its inevitable shortcomings with equanimity, Barber looks to it as an earnest model of government—our government, in fact, with the Court ruling, as Learned Hand once commented, as a bevy of Platonic guardians (with the qualification that we can at least impeach these guys [pp. 41–42, 115]). Do I exaggerate? Consider these points. First, the "heart" of the "real" constitution, is not the text; its essential dimension, i.e., what makes it a constitution, is its commitment to reason over passion and justice over democratic choice. Thus, paradoxically, "the sovereign people cannot create the Constitution [so understood]; the Constitution is rather a first step toward the people creating themselves as a sovereign" (pp. 65, 116, and 217). Second, in interpreting the defeasible text of the Constitution, in light of its essential commitment, justices should give that document the most attractive construction its language will bear (pp. 109–110). But third, when the language simply cannot be squared with the demands of justice under present circumstances, when in other words there is a conflict between its ends and its means, the Constitution becomes an incoherent measure of action. "Because an incoherent measure cannot measure, there is no possibility of constitutional action by any actor under conditions of constitutional incoherence. . . . [T]he Constitution has no prescriptive force" (pp. 207–08). Unbound by the Constitution except in its commitment to justice, the judge may then do whatever he or she thinks likely to restore a constitutionally governable state of affairs. As possible historical models of such action, Barber mentions school desegregation remedies, including the mandating of taxes for such operations.

Especially with the second and third points, it is hard to see how the Constitution could function as law rather than an authorization for the courts to lead us toward simple justice. Justices should look to constitutional provisions when they do not constrain their quest for justice, Barber seems to say, and ignore them when they do. That indeed is a Constitution of Judicial Power, but I had thought that ours is a constitution of law and of self-government.

Colgate University STANLEY C. BRUBAKER

To Make a Nation: The Rediscovery of American Federalism. By Samuel H. Beer. Cambridge: Harvard University Press, 1993. 474p. $29.95.

Samuel Beer has made many contributions to the study of politics in our time, but in this writer's opinion his most significant contributions have been his studies of federalism, to which, he says, he turned in the 1960s as a liberal concerned with the American federal union and fully supportive of its nationhood yet at the same time concerned that its states remain healthy, functioning parts of the American nation. In the years following, Beer addressed contemporary questions of federalism from his perspective, raising those issues for serious consideration on the American political science agenda.

Now Beer turns to a comprehensive study of the emergence of American federalism and the federal system to which it gave birth as viewed from his liberal perspective. Nor should we have a doubt as to how liberal that perspective is. He himself mentions at the very outset of his book the influence of Herbert Croley's *Promise of American Life* (1909) on his argument for a new nationalism. Croley, however, gives the impression of acknowledging federalism only because he had to, as something he would just as soon have done away with from the beginning—definitely not Beer's position.

As Beer himself suggests, this is a work of filial piety in the sense that he sees it as repaying a debt to his grandfathers who served in the American Civil War, in the struggle to save the Union and, as others have suggested, to serve Lincoln's purpose of transforming it from a union of states into a nation. In a sense, it is as much a product of the Civil War as we can expect from one who experienced it two generations removed.

To Make a Nation is an important contribution to the current literature on federalism. First of all, it is felicitously written, as one would expect from its author. Beyond that, it states the best liberal case for federalism. The central premise of Beer's book is twofold: (1) American federalism is national federalism, that is to say, is based on the establishment, development, and integration of a single American people (rather than a compact among the states, the theory advanced by John C. Calhoun in the nineteenth century and periodically since then, e.g., by Ronald Reagan) and (2) American nationhood is based upon federal democracy—that is, the sovereign American people organized their government on the basis of two coordinate sets of institutions, federal and state, both equally essential to American nationhood and its operationalization as democratic nationhood.

Herein lies Beer's greatest contribution for our times. His argument against the compact theory of federalism, it seems me, has by and large been long since won in the hearts and minds of the American people and most especially among American academics and intellectuals. It may resurface from time to time and its proponents may even be able to present a good case for it, but their words fall on deaf ears.

Far less appreciated is the connection between American nationalism, federalism, and democracy. That, indeed, leads to one of my problems with the book. It insufficiently recognizes the problem of centralization in American society—that American nationhood is, for all too many, no longer the federalist nationalism of Lincoln that Beer follows implicitly and explicitly, a nationalism that sees democracy as integral to it and federalism as integral to democracy. Instead, it has become a Rousseauean or Jacobin nationalism that looks only to a putative center in Washington for all significant decision making. This is as much a distortion of the nature of the American union as Calhoun's compact theory of federalism.

Beer leads us into a rediscovery of American federalism in *To Make a Nation.* In his introduction, he examines American nationhood and the three great historical challenges to it—sectionalism, industrialism and racism—concluding with a statement of the relationship of federalism and political theory. The key to this relationship is that "federalism is a not a humdrum matter of public administration but a serious question of political philosophy," which operationally means that "decentralization is constitutional, not merely statutory," requiring "the process of consent wider than and different from the process required for the ordinary statutory law of the central government" (pp. 21, 23). To build this case, Beer takes us back to the Middle Ages, beginning with Thomas Aquinas and moving to John Milton and James Harrington. In Part 1, "From Hierarchy to Republicanism," Beer rejects the premodern assumptions regarding the organic and hierarchical order of society. Part 2 is "The National Republican Revolution," Part 3, "The National and Republican Constitution." In those two parts Beer explores the federal options and the discovery of the nation and the constitutional movement from a confederate republic to a compound republic—all designed to establish a "social union" such as was first propounded by James Wilson, one of the great national federalists of the constitutional convention. Thus, Beer builds a case for American federal democracy based upon the modern republican revolution, in what he—and this reviewer—considers most important the federal and democratic dimension that gave birth to modern constitutionalism and modern ideas of liberty, both of which have served the United States well over the past two centuries.

Beer has a very good case, and he makes it very well despite some quibbles and suggested expansions that I believe would be helpful to him. Like him, I believe that American federal democracy must be taken whole—that is to say, that the American union established a federal democratic nation; that the federal government and the states are both instruments of that nation and are coordinate in that respect, to be regarded and treated as such; and that federal democracy is thus made operational through government by discussion and deliberation. To the extent that this has been true even beyond the structures of federalism, the union has been a success. To the extent that it has been true in opposition to the structures of federalism, in this my opinion, it has deviated from standards of American federal democracy that Beer sets before us and has to that extent failed, even when it has done so to make the United States a better social union, the last of Beer's points of emphasis.

Precisely because Beer's devotion of federal democracy is a liberal devotion, he demonstrates the close connection between federalism, democracy, and liberty that should lie at the basis of every liberal's creed and should shape American liberalism in the future as it has in the past. By rescuing federalism from conservatism, Beer demonstrates that for Americans, both are branches out of the same federalist trunk. The debate between them is at its most productive when it is a debate over the substance of federal democracy, not a fight between federal and Jacobin democracy as it is has become in the twentieth century. We are all the poorer for that, and we are the richer and, hopefully, the wiser for having Samuel Beer's book to remind us of that aspect of American heritage.

Center for the Study of Federalism DANIEL J. ELAZAR

State Government and Economic Performance. By Paul Brace. Baltimore: John Hopkins University Press, 1993. 152p. $28.95.

Surely one of the enduring legacies of the Reagan era in American politics has been the resurgence of the states in the federal arrangement. Although such was part of the intent that lay behind the contraction of intergovernmental aid from Washington in the early 1980s, the president could hardly have anticipated the way in which many of the states would fill the vacuum in the realm of economic development policy. As the Reagan administration preached the virtues of the free market, deregulation, and rugged self-reliance, many of the states chose the quite opposite course of active economic intervention. To encourage business investment within their borders, employment growth in industries of the future, and a steady revenue stream, state governments undertook a wide variety of strategies whose outlines suggested nothing less than the sort of industrial policies familiar to the Europeans and Japanese.

Naturally, it occurred to many observers to wonder just what it was that state governments thought they were doing. States, after all, are not nations: they cannot control their borders, their currency, or their trade. Embedded as they are in the national—and indeed, the international—economy, is it possible for states to affect their economic performance? This is the important question that Paul Brace addresses.

Brace is not the first scholar to struggle with this issue, which he calls the "mystery of state political economy." Though he begins by claiming that we have virtually no evidence that state government efforts have positive effects on their economies, there is in fact a substantial recent econometric literature, summarized in Tim Bartik's *Who Benefits from State and Local Economic Development Policies?* (1991), that demonstrates just that. Thus, what sets Brace's work apart is not that he manages to demonstrate positive state-level effects but rather that he offers us a theoretically rich state-of-the-art research design.

Brace argues that to discern state influence on economic growth, we must first control for national economic effects. It is here that the book makes a unique and important contribution. If state patterns of economic growth and decline mirror those of the nation, then we must conclude that the state economy is entirely dependent on national forces. If, however, states track distinctive patterns, then we can argue that state economies are shaped more or less independently of national economic trends. Once the degree of state dependence or independence is established, we may explore the relative influence of various factors subject to state control, such as governmental capacity, the programmatic commitment to economic development, tax policy, and political factors.

The analysis proceeds first by a series of case studies to provide a state-level view from the perspective of two low-tax, weak government, laissez-faire states (Arizona and Texas) and two relatively active intervenors (New York and Michigan). The lesson we learn here is that when the national economy is booming, as it did in the 1960s and 1970s, states with small and inexpensive public sectors did well. States with high capacity and the high taxes necessary to support it are at a comparative disadvantage in such a climate: big government acts as a brake on growth as mobile capital and labor migrate to low-cost states. But in a period in which national growth cannot be taken for granted, such as 1980s, states willing and able to invest in education, worker training, infrastructure, and economic development are better positioned to weather the economic storms.

Brace makes much of the rapid deterioration of the

Arizona and Texas economies in the last decade, as the real estate and defense economies went sour and low-wage industry migrated off shore. Unfortunately, their "endogenous political process and policies . . . appear ill-suited to compete in the more complex global economy. . . . That which served to recruit industry 20 years ago produced rates of illiteracy that are disadvantageous in a service-oriented, technologically driven economy" (p. 49). The experience of Texas and Arizona, then, suggests that economies of states with poorly developed capacity tend to ride the waves of national economic growth and decline.

The obverse of this argument is that the states whose economic performance is largely independent of national trends and that have elaborate public sectors and a history of active economic intervention did well in the Reagan years. But the fact is that many of these high-capacity states also experienced severe decline in the 1980s. Remember the demise (ill-timed for candidate Dukakis) of the "Massachusetts miracle": despite its array of innovative economic development programs put in place in the 1970s, Massachusetts could not stave off the effects of national industrial disinvestment or the defense build-down. For that matter, other aggressive economic players like Connecticut or Michigan could not do much better. Thus, the case for planned state economic intervention (which theoretically makes more rapid recovery possible) is not supported empirically by these case studies.

Brace then turns to an analysis of aggregate growth patterns from 1968–89 for the 48 continental states, first looking at trends over the entire period and then breaking the trends down in five-year segments. The latter exercise is revealing. Beginning in the early-to-mid-1980s, there appears what Brace calls a "fundamental change in the nature of political economy among American states." Although, over the longer period under consideration, national economic effects generally account for state fluctuations in value added in manufacturing and, to a lesser extent, in personal income growth, after Reagan's New Federalism reforms, the independence of the states greatly increases. Moreover, when various national economic forces such as defense spending are controlled, state capacity (e.g., economic development initiatives and tax policy) is significantly positively related to personal income growth and somewhat less strongly to increased employment. Income growth, in turn, is related to manufacturing growth, measured in value-added terms.

The bottom line, then, is that the role of the states as economic actors did change in the 1980s, just as a number of less technically rigorous studies have claimed: in the aggregate, at least, the states appear to have developed an effective array of tools and strategies for husbanding their economies.

At the end of the book, Brace is concerned about the durability of this new American political economy. He already had seen signs of eroding state commitment to the new activism. He was right to wonder; for even in the time since the book was finished, new indications suggest that the ardor that the states brought to their emerging entrepreneurial roles has begun to wane. Nevertheless, the experience of the states with economic development over the last couple of decades is an extremely important story for understanding both the extraordinary dynamism of the federal arrangement (illustrated particularly by the constantly changing roles of the states vis-à-vis Washington) and the evolving relations between American governments and the market economy. With this book, Brace takes his place as one of the most rigorous and important chroniclers of these shifts.

University of Wisconsin, Madison PETER EISINGER

Crafting Equality: America's Anglo–African Word. By Celeste Michelle Condit and John Louis Lucaites. Chicago: University of Chicago Press, 1993. 355p. $56.00 cloth, $18.95 paper.

America began, we are told, with Thomas Jefferson's enunciation of the principle that "all men are created equal," and its political history consists in the ever more perfect implementation of that putatively self-evident truth. The abolition of chattel slavery, the extension of the franchise, the recognition of women's right to full social participation, multiculturalism—all this represents the successful adjustment of reality to the demands of a precept that was fully comprehensive the moment it was articulated. In opposition to this cheering legend, Celeste Michelle Condit and John Louis Lucaites's exhaustively researched *Crafting Equality* marshals impressive historical evidence in support of the argument that the American conception of "full" equality—equality as *the* fundamental foundation and purpose of a political association dedicated to the common exploration of difference—is the late-blooming outcome of a complex encounter between Anglo–American and African–American publics. The distinctively American commitment to equality did not spring fully formed from the minds of the founders, whose conception of equality was only slightly more expansive than the very restrictive beliefs held on the subject by their British forebears. Our current usage reflects the creative energies of African–Americans as they reinterpreted the meaning of *equality* in light of their own experiences, interests, and aspirations, gradually convincing Anglo–Americans to make these innovations in linguistic meaning their own. As a result of these discursive achievements, the United States in the late twentieth century is as much African–American as Anglo–American. More broadly, the understanding that emerges from the history of how the goal of equality was "crafted" in America suggests a view of the polity as a creative *work*, reflecting the multiple ends of the various groups who find themselves on the North American continent and their capacity, through public argument and contestation, to produce themselves as a differentiated but coherent body politic.

Turning away from official policy pronouncements and canonical political theories, Condit and Lucaites survey the use of the word *equality* in public arguments over race relations in America—speeches, debates, pamphlets, journalism, and other informal public spheres. On the basis of these sources, they track the meanings attributed to *equality* in British political ideology, its modification by the American Revolution, and its further inflection by the African–American community beginning in the nineteenth century. Prior to interventions by the latter, we learn, even the relatively expansive conception of equality put in play by those dissatisfied with British rule relied on a conception of the American nation as a racially homogeneous people with a distinctive "genius for liberty," from which enslaved peoples and their descendants were necessarily excluded. Especially problematic for this logic of exclusion (the dream of expatriation was its political expression) was the presence of free blacks, who regarded America as their home and resisted efforts to resettle them elsewhere. Their response was to universalize the meaning of equality, which they claimed applied to "*all* men" and included social equality (equality of material condition as well as political rights); a conception of citizenship based on the historical fact of being in America rather than on racial determinants; and an activist role for government in promoting and defending equality in these senses.

Such universalizing was only encouraged by the disastrous conditions legitimated by the separate-but-equal compromise between white supremacists and white and black egalitarians, leading ultimately to Booker T. Washington's idea of America as a "composite nation" grounded on full social equality for blacks and whites. Here, again, the authors show that with respect to linguistic innovation, white egalitarians followed leads invented in the black public sphere, with eventual federal support from the Roosevelt and Truman administrations leading ultimately to *Brown* v. *Board of Education*. By the 1960s, the rhetorical strategies pioneered by African-American publics were characteristic of much of mainstream American political discourse.

The backlash of recent decades stems from the fact that white egalitarian sentiment did not reflect the sensibilities of most whites, leading to a failure actually to achieve equality. This, in turn, led to divisiveness in the African-American public sphere, elements of which rejected the idea of America as a composite nation founded on equality, described America as inherently racist, identified being black solely with being a victim of racism, and put forward separatist ideologies. Such an approach, Condit and Lucaites suggest, turns its back on what is most valuable about America, namely, its commitment to the pursuit of multiple ends and to balancing diverse identities and histories in the context of a political community that is seen as an evolving discursive construction responsive to its members' changing opinions and needs. Rather than mime white supremacists by insisting that America is a "white man's country," African-Americans should build on their already considerable achievement and continue to craft an America conceived as the project of integrating differences without eliminating them, with equal participation and recognition as the watchwords.

Crafting Equality is one of those books that dramatically reorient one's view of the nature of American political life and cause one to appreciate anew the meanings of America's political vocabulary. That said, there is nevertheless something suspiciously anodyne about its picture of the basic character of American political passions. The idea that the American public is firmly committed to democracy and equality is believable if one restricts oneself to the explicitly political discourses surveyed here; but a very different, much scarier, and arguably more penetrating view of the American national psyche would emerge from a consideration of, say, American fiction from Charles Brockden Brown through Melville to Don DeLillo.

More fundamentally, *Crafting Equality* rests on an understanding of political discourse that is untouched by some of the most powerful contemporary inquiries into the relationship between language and the political, such as those stemming from Nietzsche, Heidegger, and Derrida. Condit and Lucaites are committed to an understanding of political speech drawn from the ancient Greek rhetorician Isocrates, for whom discourse is seen as an instrument used to produce, fabricate, or (as they put it) "craft" a polity as a *work* shaped according to discursively established images. While this conception is salutary—inasmuch as it enables the authors to understand meaning as an emergent effect of collective conversation, rather than as a reified, statically given constant—it also leads them to embrace the idea of the polity as a work of art. Condit and Lucaites dismiss Derrida as "dangerously nihilistic" (p. ix), but Derrida's colleagues Jean-Luc Nancy and Philippe Lacoue-Labarthe might have alerted them to the nihilistic dimensions of the aestheticization of politics, which the latter show to be deeply antipolitical in character. To be sure, Condit and Lucaites's original and persuasive interpretation of the Anglo-African character of American equality stands independently of this productivist conception of political discourse. But their willingness to dismiss Continental thought of extraordinary insight and force owing to the prejudice that its lack of "pragmatism" renders it alien to their subject marks their book as American in another, presumably unintended, sense.

University of California, Berkeley FREDERICK M. DOLAN

The Year of the Woman: Myths and Realities. Edited by Elizabeth Adell Cook, Sue Thomas, and Clyde Wilcox. Boulder: Westview, 1994. 282p. $49.95 cloth, $14.95 paper.

Running as a Woman: Gender and Power in American Politics. By Linda Witt, Karen M. Paget, and Glenna Matthews. New York: Free Press, 1994. 330p. $22.95.

Nineteen Ninety-Two was widely heralded by political observers as "the Year of the Woman." In some ways, 1992 was a breakthrough year for women and politics, while in other ways, 1992 represented politics as usual and continuity with the past. *Running as a Woman* and *The Year of the Woman* analyze the distinctive factors and developments that made 1992 an unusual year for women running for office, place the gains made by women in the 1992 elections in a larger context, document women's recent political progress, and examine continuing obstacles to women's political success. In doing so, these two books cut through the media hype that surrounded the so-called Year of the Woman and contribute to a more reasoned assessment of the significance and limitations of the gains women made during the 1992 election cycle.

Both of these books examine women's involvement as candidates, voters, and financial contributors to women's campaigns; and both add significantly to our understanding of how and why 1992 became such a critical election year for women in politics. However, analysis of the 1992 elections is more central to *The Year of the Woman* than to *Running as a Woman*. *The Year of the Woman* was conceived during—and written immediately following—the 1992 elections. *The Year of the Woman* takes the 1992 elections as its central focus and uses the elections as a lens through which to view and analyze women's involvement in electoral politics. In contrast, the research that led to *Running as a Woman* was well underway prior to the 1992 election cycle. *Running as a Woman* focuses on historical and contemporary changes in women's roles in electoral politics and integrates discussion of the 1992 elections into a more general analysis of women's candidacies and campaigns.

In addition, the two books give different weight to various explanations for why 1992 was such an exceptional year for women running for office. Both books point to the treatment of Anita Hill during the Clarence Thomas confirmation hearings and the unusually large number of open seats in Congress as two of the most important factors accounting for the record numbers of women running for, and elected to, Congress in 1992. However, *Running as a Woman* seems to give more weight to the Anita Hill explanation, while *The Year of the Woman* stresses to a greater extent the unusual number of political opportunities available to women in 1992. *Running as a Woman* opens with an extensive discussion of the effect that Anita Hill had on the elections, arguing that "before Anita Hill, there was little reason to think that the change in the 1992 elections would be other than incremental" (p. 2). The effect of increased numbers of open seats is not mentioned until several pages later (p. 6). By contrast, Clyde Wilcox, in the introductory chapter to *The Year of the Woman,* gives far more emphasis to the existence of a record number of open seats and women's willingness to take advantage of those opportunities, especially in elections for the House of Representatives. Although Wilcox acknowledges that the Senate candidacies of Lynn Yeakel, Carol Moseley-Braun, and Patty Murray were motivated in part by Anita Hill's treatment before the all-male Senate Judiciary Committee, he draws upon the research of other contributors to the book to argue that the candidacies of each of these women were also motivated by unique political opportunities specific to their races (pp. 7–8).

Both books have multiple contributors. *The Year of the Woman* is an edited volume with 14 chapters, including a chapter on each of the major 1992 Senate races involving a woman candidate (California, Illinois, New York, Pennsylvania, and Washington), the House elections, state legislative elections, political parties, women's political action committees, political advertising, voting, and the impact of women public officials. About half of the contributors are established scholars of women and politics. Most of the other contributors have expertise in relevant areas of electoral politics. In general, the essays are of surprisingly high quality, given the unusually short time frame in which this book was produced and published.

Running as a Woman is coauthored by a journalist, a political scientist, and an historian. The book has both the strengths and some of the weaknesses one might expect from such an interdisciplinary collaborative effort. The differences in training and perspective result in a rich, multifaceted approach to the topic of women's involvement in electoral politics. Glenna Matthews's influence is evident in the way that women's contemporary involvement in politics is placed in a larger historical perspective. For example, an early chapter in the book traces how women came into office historically, how they derived their public authority from their relationships to men, and how this pattern changed over time. Similarly, Linda Witt has clearly contributed numerous richly described anecdotes that perfectly illustrate critical points. Karen Paget's analytical skills and training as a political scientist are also evident, perhaps most obviously in a chapter on mobilizing women's votes. Nevertheless, the very fact that one can attribute certain sections or elements of the book to one coauthor or another highlights a weakness of the book: multiple voices are still evident, and the styles do not flow together as smoothly as one would find in a single-authored work.

The major argument of *Running as a Woman*— and one well substantiated by the material presented in the book—is that recent elections, particularly the 1992 elections, have witnessed a shift in strategy on the part of women candidates. Most women have traditionally run for office by emphasizing their similarities to men, and consultants have advised women candidates not to call attention to their femaleness. However, in response to a changing political environment, more women in recent elections have chosen to run as women. For example, in 1992 women candidates often focused on the need to elect more women public officials, emphasized women's issues, and targeted women voters.

Some of the topics examined in *Running as a Woman* are familiar ones that have received considerable attention in other works focusing on women candidates. In some respects, *Running as a Woman,* especially in the chapters on squaring the personal and the political and funding women's campaigns, provides a 1990s update to Ruth B. Mandel's *In the Running: The New Woman Candidate* (1981). The gender gap in voting and the impact of women elected officials—two topics to which chapters are devoted—have similarly received serious and sustained examination in other works. However, this book also includes chapters on topics that have received little attention in previous books on women candidates. One particularly original chapter examines how the legacy of Jezebel—"a woman whose sexuality threatened the well-being of the community" (p. 54)—has shaped perceptions of political women and has affected their public presentations of self. Other chapters focus on risk taking and losing and on press coverage of political women—important topics that have been largely overlooked in previous works on women and politics.

Judging from the notes, the coauthors of *Running as a Woman* conducted interviews with numerous well-known women public officials, candidates, political consultants, and leaders of women's political action committees, and other women's organizations. Because Witt, Paget, and Matthews have clearly interviewed an impressive array of people in gathering material, one wonders why their text does not include any explicit discussion of methodology or even any mention of whom they interviewed and why.

The essays in *The Year of the Woman* are largely descriptive. (An exception is the chapter by Sue Tolleson Rinehart, who frames her analysis of the California Senate races with a useful discussion of the distinction between sex and gender and their relationship to virtual and substantive representation.) Nevertheless, as a whole, the essays provide a balanced, comprehensive, in-depth examination of the many different ways that gender had significance for the outcomes of the 1992 elections. The important role that the mobilization of women as voters and financial contributors to women's campaigns played in the election of record numbers of women to office in 1992 is a common theme that emerges across several of the chapters.

Perhaps because of the short time-frame in which this edited volume was produced, most of the essays in *The*

Year of the Woman are based on newspaper reports, observation, and presentation or analysis of publicly available data. Few are based on original data collection. For example, of the five chapters examining U.S. Senate races in specific states, only Jean R. Schroedel and Bruce Snyder's chapter on Patty Murray's race appears to be based, in part, on personal interviews. The most extensive original data collection was done by Leonard Williams, who, in his chapter on political advertising, presents findings from a content analysis of 59 campaign advertisements from U.S. Senate races in 12 states. Elizabeth Adell Cook, Mary E. Bendyna, and Celinda Lake do examine data from the 1992 exit poll conducted by Voter Research and Surveys in their chapters on voting, providing some very interesting insights about women voters' reactions to women candidates and presidential candidates; and Robert Biersack and Paul S. Herrnson analyze Federal Election Commission data in their chapter on the role of political parties.

Both *The Year of the Woman* and *Running as a Woman* are excellent choices as supplemental textbooks for courses on women and politics or campaigns and elections. While there is some overlap in content, the two books would complement each other well as classroom texts. *Running as a Woman* is full of anecdotes and examples to engage students, and it raises most of the issues about women candidates and their campaigns that one would wish to address in a classroom setting. *The Year of the Woman* also examines many critical aspects of women's involvement in contemporary electoral politics but in a more academically rigorous fashion. Scholars, as well as students, will find both books useful; and both will be frequently cited in future research on women and electoral politics.

Rutgers University SUSAN J. CARROLL

Representations in American Politics. Edited by Frederick M. Dolan and Thomas L. Dumm. Amherst: University of Massachusetts Press, 1993. 296p. $40.00 cloth, $16.95 paper.

Republic of Signs: Liberal Theory and American Popular Culture. By Anne Norton. Chicago: University of Chicago Press, 1993. 195p. $34.00 cloth, $12.95 paper.

There is a good deal of overlap between these two engaging, if occasionally dispiriting, books. Anne Norton supplies herself and the editors of *Rhetorical Republic* (to which she contributes a chapter) with an organizing theme, namely, struggles over the "representations" that do, should or ought not govern politics and culture in postmodernist or late modernist polities. America is the primary empirical focus of these reflections, but the American polity and culture are also treated as the paradigmatic instance of the wider phenomenon taken, somewhat problematically, to be signified by these terms. Historically, phenomenologically, and perhaps "quasi-ontologically" (Derrida), the United States is a "republic of signs" or "rhetorical republic," a polity and culture that are "governed" not by material realities, institutional structures, or even ideas or ideologies but by signifiers and representations.

This theme has two sometimes dissonant strands or elements. Metatheoretically, it opposes empiricist or otherwise realist views according to which there are extralinguistic realities or canons of reason that could and should discipline rhetorics and representations. Substantively, however, numerous of the representations that in fact "govern" America are judged to be regretable in character. Although officially denying that there are divine, natural, or rational signifieds that ought to control our signifiers and representations, several of the authors complain, mournfully and occasionally bitterly, that the representations that regulate America endanger or have already destroyed estimable potentialities.

The second of these tendencies makes a qualified appearance in Dolan and Dumm's introduction to *Rhetorical Republic*. They acknowledge the impossibility of representations that govern in the strictest sense of logically, ontologically, or otherwise reducing us to one set of possibilities. In principle, all signs and signifiers—and hence all representations—are subject to contestation. But there is a "will to impose" exclusive representations, and it frequently succeeds in stilling competition over them. All of the essays in part 1, "Cold Wars" (though treating many events of death and destruction) and two in part 2, "American Alterities," hammer on this anvil. Avital Ronnel's psychoanalytic reading treats Desert Storm as George Bush's war of termination with himself but also as characteristic of a tendency to despotic closure that Ronnel thinks is evident in democratic theory and practice at least since Kant. David Campbell's powerful "Cold Wars: Securing Identity, Identifying Danger" claims that America manifests an aporia inherent in the identity of republics, one that typically is "closed" by constructing, however fancifully, enemy others. Variants of this argument are also advanced in a Girardian idiom by George, Derrian, and Honderich with reference to the war against drugs; in Diana Rubenstein's Lacanian assessment of Oliver North, Iran/Contra, and national security operations generally; in Stuart Clarke's Gramscian dissection of the Willie Horton and Clarence Thomas episodes; and perhaps even in John Dolan's lament that protest poetry is tiresome in its liturgicality.

On these views, then, the American polity and culture are governed by representations that are effectively hegemonic. In these papers, what I call the "metatheoretical thesis" is employed primarily to claim that the authors themselves have means sufficient to enable them, apparently quite readily, to see through—to unmask—governing representations. (Occasionally an excess of deconstructive and especially psychoanalytic enthusiasm yields intimations that the unmasked representations are not only not true but are *false*, that they distort or misrepresent a reality that the critic has apprehended and that warrants the critique. Charitably construed, these moments are lapses.)

Anne Norton sometimes participates in the tendencies just discussed. She argues, for example, that American liberal politics and culture features a governing "presentation" of women that subjects them to a "feudal" position or situation (*Rhetorical Republic*, pp. 126–29; *Republic of Signs*, pp. 47–48, 54). For the most part, however, the *Republic of Signs* is more nuanced, adventuresome, and even optimistic than the essays. In a "liberal regime of representations," all representations—all signs and signifiers—at once constitute and provide means of challenging and reconstituting their subjects or signifieds. They are "duplicitous" not in the sense of misrepresenting or lying about some prediscursive reality but in that they enable and invite a doubling, a more or less continuous movement between passively being made by and actively making, being overcome and overcoming. The activities of naming and representing presuppose a more or less stable setting (the "unselfconscious commonalities of liberalism"); but within such a context, representations re-present not in the sense of presenting again that which has already been established or institutionalized but in the more creative sense of remaking, reconstituting.

Americans are a "people of the text," a people who wrote themselves into being in documents such as the Mayflower Compact, the Declaration of Independence, and (especially) the Constitution. But these and the innumerable less celebrated texts that make up the popular culture (a culture that Norton reads closely and creatively), while having largely made Americans into what they thus far have predominately been, are equally devices enabling Americans to contest and remake what their own texts have represented them as being. In discussions that range from the Constitution and the presidency to money and shopping, voting, lotteries, and survey research, Norton discerns and imaginatively invents possibilities that exceed recognized actualities and already approved opportunities.

Norton's evident affection for America and for liberalism at the best they have been, are, or could become provides a counterweight to the negativity of numerous of the essays in *Rhetorical Republic*. The most engaging of its essays—those by Chaloupka, Honig, Villa, and Connolly—share the inveterately unstable combination of critique and affirmation that Norton seeks and (occasional anodyne moments aside) for the most part sustains.

The Nortonian themes common to the essays can be signaled by the terms *refusal, resistance,* and *augmentation.* We should refuse and resist the numerous racist, sexist, imperialist and otherwise overweening tendencies of our liberal American polity and culture. And we should strive to augment the best parts of what has acquired authority or achieved authoritative standing in that polity and culture. Because our culture is replete with ambiguity and dissonance, because it is a liberal culture as well as a republic of signs or representations, it affords resources necessary to critical and constructive initiatives.

In a spirited and compelling counterpoint to widely received interpretations (e.g., Gitlin's) of New Left and later street politics, William Chaloupka reads them as Arendtian theatricalities, as performatives whose "speed" at once depends upon and exceeds the possibilities afforded by generally accepted ideologies and institutionalized structures and roles such as electoral politics and citizenship. It may be true that the "cheeky tynics" (Sloterdyk) displayed in the spectacles they staged were lacking in the definite, substantive goals widely demanded of political actors; but they drew upon and revivified the noninstrumental idealism that Norton shows to be liberalism at its most exalted.

Arendtian themes are yet more explicit and prominent in the essays by Honig and Villa. Honig reads Arendt as attempting to invent representations that ought to govern the American polity. In particular, she argues that Arendt sought to theorize a practice of authority suitable to a politics that must do without foundations, that can have no fixed or unalterable basis or bases. The authority of the Declaration and the Constitution was created by performatives such as "We hold, . . ." but this very fact about them requires that the "truths" thereby constated must be viewed as continuously resistible and constantly in need of augmentation. Similarly, Villa argues that Arendt invented a conception of "the public sphere" that features a "politics of parody." Precisely because it would eschew epistemological and ontological certitudes—would self-consciously internalize resistance and self-critique—such a polity and politics would be protected against the disillusionment and delegitimation to which truth- or reality-claiming regimes are so evidently vulnerable. A republic of signs, representations, or appearances would be, at once and for the same reason, better secured and more fecund than a republic of signifieds.

Chaloupka, Honig, and Villa, inspired by Arendt, seek to make a friend of what many will regard as the enemy, that is, radical uncertainty and indeterminacy. A politics unconstrained by law, truth, or reason would give wings to the human spirit. In his lively argument for a politics that exceeds territorial boundaries, William Connolly is moved by the same idea/ideal but pursues it beyond the limits that Arendt explicitly endorsed. Arendt preceded Connolly in her disdain for the notion of sovereignty. But an Arendtian politics presupposes what Villa calls "spatiality," that is, a community of people bounded by geography. Connolly's insistence on the importance of institutionally based "electoral accountability" sustains, nervously, more than a remnant of this assumption. In the name of a democratic politics appropriate to late modernity, however, he urges us to think of the signs or signifiers that delineate states as every bit as open to contestation and resistance as all other putatively final markers. He concedes (at last!) that "democracy . . . is a form of *rule* or *governance*" (and hence must often be resisted) but argues that it is (might be?) also "an egalitarian constitution of cultural life" that should no more be confined spatially or territorially than it should be circumscribed by natural law or class and caste structures.

If I have a general hesitation concerning this last group of essays, it concerns their sometimes intense valorization of the notion of politics. Honig reminds us that whereas, for Arendt, politics is a creative and humanizing activity that "*ends* with the entry of the . . . absolute," on Derrida's Weberian view "politics *begins*" when absolutes are asserted and especially when attempts are made to impose them by authority or power. On the latter view, politics is much more a matter of resistance than of creation. We turn to politics to protect our creative endeavors from threats launched by the state and other aggregations of power. This view—which I think can be discerned in *Republic of Signs*, perhaps in Chaloupka's essay, and possibly even in Connolly's on egalitarian cultural life—seems to me to be the more estimable of the two.

The quality of the thinking presented in these two volumes helps to explain—and will help to sustain—the gratifying circumstance that numerous talented young people are now attracted to that nexus which is called political theory. It would be pleasing to think that this is because these books, like much in the recent literature, are pervaded by the spirit of one of Wittgenstein's choicest remarks: "When one draws a boundary it may be . . . to prevent someone from getting in or out. . . . But it may also be a part of the game [that] the players be supposed . . . to jump over the boundary."

Johns Hopkins University RICHARD E. FLATHMAN

Bitter Fruit: Black Politics and the Chicago Machine, 1931–1991. By William J. Grimshaw. Chicago: University of Chicago Press, 1992. 248p. $24.95.

Since the 1960s political coalitions throughout the country have struggled with some success to overcome white racial resistance by capturing the political resources of city politics. Population shifts have rewarded black voters in their attempts to turn conservative coalitions out of office in city after city over the last thirty years. As the Republicans turned to the suburbs to capture national politics, central cities remained the last bastions of liberal Democratic reform. Recent fortunes have not been so kind to liberal reform. Writing at a time when black dominant coalitions have been overturned in New York, Los Angeles and Philadelphia, William Grimshaw provides a timely and richly documented account of how black politics, Chicago style, though it participated in the rise of the Daley machine and was responsible for its fall, was unable to institutionalize a reform tradition to succeed the machine era.

How did blacks face an era of machine politics? The answer, according to the author, is with an awareness that if you were black you could not necessarily depend on the traditional adage that votes would translate into material rewards. Grimshaw questions the dominant model of machine politics, which posits that the machine attracted black voters by simply exchanging economic benefits for political support. Instead, turning the "economic" model on its head, Grimshaw argues that even though the Daley machine largely owed its success to black support, the black community received only "bitter fruit" in return.

The book begins as a critique of the economic model in which machine politicians are assumed to hold a "unitary" interest in winning elections and distributing patronage. Machine politicians are viewed by Grimshaw as operating within a loosely coupled system seeking to win elections by building coalitions among factional rivalries in order to expand their bases of power. When applied to Chicago, the economic model, according to the author, oversimplifies a complex political process that resulted as much from critical local and national events as from assumptions concerning group and elite calculi.

The author argues that from the perspective of citizens the study of machine politics requires a distinction among three concepts: exchange, representation and empowerment. Machines grant small favors in exchange for political support but generally avoid substantive representation. Mayor Daley often provided symbolic representation in order to quiet ethnic and racial rivalries within the organization. Substantive representation that moved toward power sharing was an anathema. And, "needless to say, if the machine looks unfavorably on demands for representation, it abhors any demand for empowerment." Daley heeded black politicians when he needed black support as a way to gain control of the organization. Once gaining control, he often successfully coopted black loyalists who sought benefits in return for political support and shunned those who pushed a reform agenda. His use of William Dawson in order to control black votes became legendary, though "Boss Dawson" was less powerful than often portrayed, in that his power waned through successive battles within factions of the governing coalition. The machine was not monolithic. Even though it was hierarchical, it was filled with factions.

Grimshaw offers a novel interpretation for why black support of the machine came relatively late and was selective. Rather than responding to machine incentives at the local level, black voters favored machine candidates only after the new deal realignment occurred at the national level. Middle class wards in particular had voted less regularly for machine candidates than lower income wards and often provided leadership to resist machine influence. Once racially restrictive housing covenants were outlawed in 1948 middle class blacks began moving out of the black belt providing a more stable base of support for the machine among lower income black voters. Ironically, Daley first ran a populist campaign with black support against downtown business interests in a racially charged campaign organized by the incumbent Mayor. Black support was important to the stability of the early Daley era. Between 1955 and 1963 the Mayor sought to co-opt black leadership by appointing "civic notables" to boards and commissions. After that point, with rising civil rights protests the Mayor subverted efforts to increase black representation and political incorporation by appointing "loyalist" Catholic black leaders who were more insulated from the essentially Protestant Civil Rights movement and held closer ties to the machine's Irish Catholic leadership.

Machine resistance to black empowerment began to crumble as the national civil rights movement gained momentum in the mid-sixties. Grimshaw skillfully demonstrates how middle class black wards began to lay a basis for the movement from machine politics to racial politics, eventually resulting in the election of Harold Washington as Mayor in 1983.

Electoral support for the machine began to unravel with the death of Mayor Daley in 1976. A convergence of forces including growth in the percentage of the voting age population who were black as well as growth in white liberal and black middle class commitment to a more redistributive political agenda resulted in an unprecedentedly active reform electoral mobilization effort around Harold Washington's candidacy. Given these forces, it is still questionable whether Washington would have won the election if it had not been for the lucky accident of an opposition in disarray. Even though Mayor Washington's reelection in 1987 was heralded as proof of the demise of traditional machine politics his untimely death provided an opportunity to reinstate the long standing dominant coalition dedicated to limiting reform efforts by those who would push even a mildly redistributive political agenda.

If the experience of other cities is a guide, the author is right in his conclusion that until blacks come together in a multi-racial coalition with their Hispanic and white liberal allies they are unlikely to be able to reestablish the reform agenda in Chicago. Multi-racial electoral coalitions, as the Chicago experience suggests, though difficult to build and sustain offer the most promise for an increasingly responsive city politics.

San Francisco State University David H. Tabb

Echoes of Discontent: Jesse Jackson, Pat Robertson, and the Resurgence of Populism. By Allen D. Hertzke. Washington: CQ Press, 1993. 293p. $29.95 cloth, $19.95 paper.

In this thoughtful and well written book, Allen Hertzke takes the unconventional view that Jesse Jackson and Pat

Robertson are more similar than stereotypes might suggest. Hertzke argues that the two preacher-politicians in fact have much in common, having combined prophetic preaching with church-based political organizing to change dramatically the character of American presidential politics in our time. Moreover, he contends that although they appear to be ideological opposites, they can be seen as sharing a similar political perspective, a tendency to see politics as a process in which discontent, resentment toward elites, and faith in charismatic leadership are channeled into a religiously-based brand of popular protest that he calls "gospel populism." To describe this shared populist strain, the author draws on a diverse set of methods and sources: analysis of biographical, historical, and theoretical texts; extensive interviews with Jackson and Robertson as well as with many of their advisors and selected followers; interpretation of survey data from state and national election studies; and firsthand observation of the Jackson and Robertson campaigns over a four year period from 1988 to 1992. He combines these approaches carefully and at times elegantly, and the result is a book that is scholarly, thought-provoking, and readable, and that would be an excellent choice for classroom use.

At the heart of the analysis is the concept of gospel populism, which is described as a paradoxical blending of economic progressivism and social conservatism. Avoiding the once-popular social and psychological explanations that depicted populist politics as the product of authoritarian paranoia or anxiety about social status, Hertzke goes deeper to place populism in cultural and historical context. He finds its roots in the rural and small-town localism of eighteenth-century America, and follows the growing gap that opened over the course of the nineteenth century between this Christian communitarian culture and an increasingly cosmopolitian and liberal one. The diverging strains came into conflict at the close of the century, giving rise to the populist movement and to William Jennings Bryan, whom Hertzke chooses—a bit too blithely given the atypical character of Bryan's brand of populism—as representative of populist sentiments. Advocate of child labor legislation and enemy of evolution, champion of pacifism and prohibitionism, feminist and fundamentalist, Bryan embodied the paradox of economic progressivism and moral traditionalism, of protest and piety, that shaped gospel populism. So too do Jackson and Robertson, as Hertzke describes them: Jackson the economic egalitarian who denounces the abuse of drugs and decries the decline of the family, Robertson the moral conservative who chastizes corporate elites for their greed and champions the Calvinist virtues of Main Street over the conspiratorial schemes of Wall Street. At times the argument is overstated, as in the description of Jackson's agony over abortion, which the author claims he opposed for religious reasons but supported for political ones. In the same way, it is possible to make too much of Robertson's rantings about conspiracies of international bankers and their allies on the Trilateral Commission. At times, Hertzke seems to concentrate too much on similarities of style and too little on differences between the policy positions of the candidates. Nevertheless, the comparison is enlightening, and the book's most important contribution is the insight it offers into the complex character of these politicians.

After a discussion of the development of Jackson and Robertson as religious and political thinkers, the book goes on to link personal and intellectual considerations to political and institutional ones. In a series of chapters, it considers the role of churches in the 1988 presidential campaigns, examines the assimilation of religious activists into the major parties, and surveys the characteristics and opinions of Jackson and Robertson voters. In different ways, these chapters treat the tensions that existed in these campaigns between church-based and party-based politics. Borrowing from resource mobilization theory, Hertzke compares Jackson's reliance on the black church—"the church as precinct"—to Robertson's use of television and mass mailings—"the charismatic network"—and he presents both as threats to established party politics. Contrasting Rainbow Coalition with Christian Coalition, he describes the moral motivations that bring the activists in these organizations to politics, elaborating on the costs and benefits that come to the parties when they recruit them. Finally, interpreting information taken from state and national election surveys, Hertzke shows that Jackson and Robertson supporters have much in common, being as a group more typically female, minority, and working class than the backers of other more conventional candidates. The comparisons go only so far, and Hertzke allows that while women supported the two candidates, they did so for dramatically different reasons, and that Jackson backers took ardently feminist stands while Robertson supporters tended decidedly in the opposite direction. Even so, he is convincing in his claim that the two groups are a lot alike, that they share a religious world view, a perception that elites are not listening to them, and a pessimistic assessment of the future of American politics.

In concluding, Hertzke speculates about the future, and here his argument becomes more challenging and potentially more controversial. Admitting that populist movements usually fail, but citing survey findings that the American electorate seems increasingly alienated and angry, he assesses the potential for a broadly based "resurgence of populism." Here the analysis becomes disconcertingly broad and blurry, including references to recent "populists" such as John Silber, Patrick Buchanan, and David Duke, along with Harris Wofford, Jerry Brown, and even Bill Clinton. Yet by expanding his focus, Hertzke can caution against the dangers, particularly the dangers of anti-Semitism and racism, that can follow from a politics of discontent. Furthermore, he can remind readers of the more positive potential for contemporary populists to take stands that are both egalitarian and morally virtuous. In this regard, he makes an articulate argument that populists of left and right can find common cause in supporting the economic and moral welfare of the nation's children. Ultimately, Hertzke is realistic, resigned to the fact that the promise of populism remains unfulfilled, an echo of discontent. But he is also hopeful that some brand of populist communalism can provide an antidote to the anemic state of American politics, and his book offers a sensible vision and an strong voice in support of a more economically secure and more morally sound politics.

University of North Carolina, Chapel Hill MICHAEL LIENESCH

Legitimacy and History: Self-Government in American Constitutional Theory. By Paul W. Kahn. New Haven: Yale University Press, 1993. 260p. $27.50.

The Partial Constitution. By Cass R. Sunstein. Cambridge: Harvard University Press, 1993. 414p. $35.00.

Both of these books are sharply focused and forcefully argued. Both are by distinguished authors covering much the same ground and are based on broad understandings of their subject matter. As a result, both will well repay careful reading. This is especially true of the Kahn book; because of the power and consistency of its analytic scheme, a course in American constitutional theory could be taught straight from its pages. Sunstein's argument is more particular, but many of his arguments and commentaries would "backstop" such a possible course wonderfully. Yet there is about both these books a tinge of unreality, almost of political naïveté, that must detract from their shared enthusiasm for deliberative (or "discursive") democracy (or "liberal republicanism"). The enthusiasm itself cannot be questioned. The relevance of this vision of democratic life to the extant American political/constitutional system *can* be questioned—and sharply.

Sunstein has the more explicit statement about deliberative democracy's meaning, for defining its terms is central to his argument. He opens his book with what becomes one of its major themes, a sharp attack on the principle of neutrality by reference to which the Supreme Court, especially in cases involving constitutional review, tries to settle matters in ways that disturb the status quo and its given distributions of power and material benefits as little as possible. Any alternative course, it is often supposed, would necessarily involve some measure of judicial "activism" and "partisanship" on behalf of groups urging change. But, argues Sunstein, the given social order is not given as if from nature itself: it is a social/legal/constitutional construct as much as any other. Moreover, any decision to sustain it is an act of impartiality that not only may perpetuate long-standing injustices but also is a prejudicial judgment that certifies the status quo as legitimate by reference to criteria that are external to itself. The appeal to external standards is inevitable, for no constitutional system can self-legitimize. Sunstein would much prefer that the Court be self-conscious and open about this process and (again, especially in its constitutional review work) strive to render decisions that show a consistent partiality to realizing more perfectly the founders' central original aspiration for the United States, namely, that it should be a *deliberative* democracy.

Sunstein's general account of this concept is simply that it requires that policy decisions should be justified and shaped by reasons grounded in appeals to the public interest and not by reference to the naked self-interest of particular groups and their raw political power. In chapters 5 and 6, the heart of his book, Sunstein articulates this general conception in terms of four further principles: (1) a commitment to the notion of political deliberation itself, what used to be called "government by discussion," (2) pride in partipatory citizenship, (3) celebration of agreement as a regulative ideal and, (4) the need for genuine measures of political equality. In his treatment of these matters, Sunstein is always careful to point out that the other branches of the government and local government should do more to realize America's democratic ideals than the courts. Nevertheless, he gives over the whole of the second part of his book to showing how a conscious commitment to deliberative democracy would force the court to change (or at least reargue) many of its decisions in areas ranging from free speech, to reproductive rights, to education.

Even if with considerable pessimism, Kahn argues in his book to something like the same conclusions. His analytical scheme arranges the history of American constitutional theory in a logical progression. The first stage has the founders designing a constitutional structure in terms of a rational response to an agreed political science. The Marshall court, especially, followed suit; and its great decisions are best regarded as evolutions from the same political science. The next stage sees the court now deferring reverently to the word, spirit, and intent of the founders simply to conserve their work. This stage is followed by a more erudite one in which the court, much guided by academicians, sought to place itself and its decisions on the high road of social evolution toward civilization (a self-understanding that to me sounds more like Hegel than Darwin, especially in reference to Woodrow Wilson). The modern period sees a split between the academy and the courts, with the academy increasingly preoccupied with evolving theories of discursive democracy (there is a discussion of Sunstein's earlier work) and the Court more and more locked in the paradox of individual autonomy versus majority rule. Neither side, in Kahn's view, has been able to solve what is essentially a problem of political obligation: Why should a democratic people empowered in the present moment honor a court (and its power of judicial review) whose legitimacy is based on history?

My doubts about these books are essentially two. One has to do with how deep their understanding of constitutional theory goes. Neither book addresses questions about the role of the courts in a democratic society in a manner or profundity that would show that they had learned anything from Aristotle or Hobbes. More particularly, Kahn's analytical scheme seems more to march around an unexamined bedrock of understanding about the nature of courts, adversary proceedings, and the functions of judges and the law in them than to establish any deep line of progression. My second doubt is directed toward their shared understandings of the aspirations of the founders and of the democratic character of the constitutional structures they created.

Both books assert that the founders subscribed to the doctrine of popular sovereignty. They probably did, but the phrase has a double meaning. On the other hand, it refers to that general popular acceptance and daily relegitimization which any constitutional republic, regardless of its particular characteristics, must receive simply to survive. Citizens provide this not just when they vote but whenever they obey the law in any way. The American founders understood and accepted that understanding of popular sovereignty soundly. Whether they accepted the phrase in a second and narrower sense that would have "the people," by deliberate constitutional provision, involved centrally and on a near-daily basis in the policy making and implementation of the government can be doubted. Before even the House of Representatives can be judged discursively democratic (at least in aspiration), the founders' theory for that body needs to be stated explicitly and related to its constitutionally provided characteristics; and the behavior of the House today would have to be related and understood.

The books fail on these two counts, nor do the authors show any conscience about it. Their enthusiasm for discursive democracy transcends: it is as if they had never heard of Rostenkowski—not the congressman indicted for corruption but the chairman of the House Ways and Means Committee, who would have to be invented if he did not exist (as often enough he does not).

New York University H. Mark Roelofs

Rethinking Constitutional Law: Originalism, Interventionism, and the Politics of Judicial Review. By Earl M. Maltz. Lawrence: University Press of Kansas, 1994. 148p. $27.50.

The Constitution and the Courts: Law or Politics? By Michael J. Perry. New York: Oxford University Press, 1994. 227p. $35.00.

Whatever its merits in law or politics, the 1986 nomination of Judge Robert Bork to be an associate justice of the Supreme Court was a definite boon to the cottage industry of constitutional law scholarship. By endorsing the idea that courts should enforce the framers' original conception of the Constitution, Bork created shock waves that scholars are still trying to absorb or deflect.

Both Maltz and Perry—longtime combatants in the battle over constitutional interpretation—take on Bork in their new books. Maltz's concern is to critique all sides in the noisy debate, urging theorists of both the Left and the Right to "abandon conventional wisdom and reorient their thinking in new directions" (p. 108). Perry takes on the more ambitious task of harnessing the theory of original intention to his own purposes, which are decidedly different from Bork's. Both books are admirable examples of judicial scholarship; but after reading them, one longs for a cessation of the Constitution wars or, at least, for a UN-imposed cease-fire.

Though Maltz and Perry attack the problem of constitutional interpretation in very different ways, both keep Robert Bork very much in their sights. Both in his confirmation hearings and in his subsequent book, Bork advanced the theory of "original understanding" as the only defensible approach to judicial interpretation of the Constitution. Bork's view, in brief, is that judges should endeavor to ascertain what the Constitution was understood to mean in 1789 (or, in the case of amendments, whenever they were adopted). The theory of original understanding is not exactly the same as the theory of "original intention," since the people who ratified the Constitution may have had a different interpretation of the document than the framers themselves. In practice, however, this distinction is of little consequence.

Determining the framers' original intention, of course, is fraught with practical problems, not least of which is the obvious difficulty in agreeing on exactly who qualifies as a framer. Bork's answer is to endorse a theory of judicial minimalism: when judges cannot determine the original understanding, he argues, they should simply refuse to enforce the decree at all, leaving the field open for legislative and executive action. From Bork's point of view, in other words, an ambiguous constitutional provision might as well not be there at all. And since the most important constitutional provisions are ambiguous, that would mean a shift away from judicial activism and judicial intervention—lo and behold, exactly the result endorsed by conservatives like Bork!

Both Maltz and Perry point out that the minimalist approach does not follow automatically from originalism. On some matters (e.g., the First Amendment) an originalist might well justify a strong judicial role in restraining government power. In fact, the originalism/nonoriginalism debate is almost wholly separable from the parallel argument on minimalism/interventionism. The first reflects different approaches to constitutional interpretations; the second, different attitudes toward the appropriate use of judicial power.

Maltz is content to the analyze these and other issues and to find fault with scholars on all sides of the debate. He discusses not only originalist defenses of judicial review but also nonoriginalist approaches, including those that derive judicial review from democratic theory, from the need to restrain the federal government, and from the nature of the judicial function.

A list of those whose theories Maltz dismisses would read like a *Who's Who* of American constitutionalism: Leonard Levy, John Hart Ely, Herbert Wechsler, Cass Sunstein, even Madison and Hamilton. When Maltz's ax falls, moreover, it falls with finality. Arguments "cannot be defended," they "collapse completely," they rest on "fundamental misconceptions" (pp. 31, 42, 63). Much of Maltz's analysis is sound, and his shredding of contemporary judicial scholarship is at least partially justifiable. But critics should keep in mind that pointing out the flaws in complex and difficult arguments is far easier than building them up.

Perry, by contrast, seeks not only to discredit "Bork & Co." (as he calls them) but also to construct his own theories of constitutional interpretation and judicial intervention. He begins by accepting originalism—not least because no other justification of judicial review would be credible in the American context. Thus, a judge's first task is to reach "a plausible conclusion" about the "constitutional directive" that underlies a given constitutional provision. When there is more than one such conclusion, the judge must choose among them. One factor likely to influence this decision is the judge's own view of the proper judicial role: a judge who is uncomfortable with judicial intervention might choose differently from one who takes the opposite approach.

By conceding that the Constitution might plausibly have more than one meaning, Perry deftly sidesteps the most serious objections to originalism. By doing so, however, he also undermines much of the appeal of originalism, which lies in its assertion of a clear and controlling constitutional text. In any event, Perry does not stop there. A judge who has reached a plausible originalist interpretation is then free to engage in "constitutional specification"—to decide, in other words, what the constitutional provision means in a given case or context. Constitutional specification is essentially a matter of "political–moral judgment"; and since judges excel at such judgments (or at least do better than ordinary politicians), they might as well have a go at it.

Perry supports his argument with specific examples from the Fourteenth Amendment. Though the historical record is long and ambiguous, Perry has little difficulty deciding the original intention of its drafters was to ban discrimination against members of any group "if the group is defined, explicitly or implicitly, in terms of a trait irrelevant to their status as human beings" or "if the group is defined, explicitly or implicitly, in terms of an activity, a way of life, or a set of beliefs—whether that activity, way of life, or set of beliefs be religious, political, cultural, etc.—towards which no state may, as a constitutional matter, express hostility" (pp. 130–31). This grand conclusion is defended in a total of two paragraphs; while plausible, it is hardly self-evident (no matter, though, since judges need only reach a "plausible conclusion" about the meaning of the Constitution). Then the real fun begins. Perry suggests that the *Roe* v. *Wade* was correctly (or at least plausibly) decided, albeit on the wrong grounds, while *Bowers* v. *Hardwick*, in which the Court refused to extend Fourteenth Amendment protection to gays and lesbians, was not. The result may not be compelling, but one must admire Perry's dexterity in getting there.

After reading Perry, it is easy to sympathize with Maltz's conclusion that modern constitutional scholarship has reached a theoretical dead-end. "Proceeding from widely shared but fundamentally misconceived premises," Maltz writes, "originalist and nonoriginalist analysts have been equally unsuccessful in creating intellectually satisfying models describing the appropriate role of judicial review in the American governmental system" (p. 108). In fact, the situation is bleaker than Maltz suggests, since his optimism that progress can still be made if scholars "abandon conventional wisdom and reorient their thinking in new directions" is undermined by his own argument.

Perhaps it is time for constitutional scholars to accept the fact that trying to justify judicial review from first principles is the political science equivalent of constructing a perpetual motion machine. The best that scholars can do is to sketch out plausible alternatives, solving nothing but giving judges of the Left and the Right intellectual support for their competing interpretive styles. Such a result might be good politics (Perry, in effect, can cancel out Bork); but in intellectual terms, all it does is make the waters even muddier.

Judicial review exists not because it is theoretically necessary or even because it is theoretically superior to any alternative. It is a kind of historical accident, the fortuitous by-product of colonial America's strong identification with the common law tradition and its invention of the written constitution as a way of limiting government power. It grew through careful cultivation over many decades by the Supreme Court and by the other branches, who found it an essential tool for the development of national strength and sovereignty. Above all, it flourished because it fit perfectly with what Robert McCloskey called "the deep-seated inclinations of the American political character," which refused to choose between the notions of popular sovereignty and fundamental law (*The American Supreme Court* [1960]).

The justification for judicial review thus lies in history, not in theory. And while the justices of the Supreme Court might benefit from following the academic debate, they would benefit even more from careful attention to the Court's past and to its ever-evolving role in the American political system. If Maltz is correct that Perry and the other theorists have failed to win their war, it may be because they have been fighting on the wrong terrain.

Clemson University William Lasser

Cooperative Pluralism: The National Coal Policy Experiment. By Andrew S. McFarland. Lawrence: University Press of Kansas, 1993. 219p. $29.95 cloth, $14.95 paper.

"What would happen if groups negotiated public policies among themselves, without relying on the state?" (p. 151). Probably most citizens, as well as many political scientists, would recoil with horror at the very thought: Allow the "special interests" to cut their own deals, with no representation of the public interest? Yet this is just the circumstance that Andrew McFarland explores in this book, and his conclusions are remarkably positive about the potential of such a process for enlightened policymaking. He is less sanguine about the political problems that can interfere with the implementation of such an approach.

The book is primarily a case study of the National Coal Policy Project (NCPP), a collaborative effort in 1977–78 between representatives of industry and environmental groups to forge national policy proposals on the mining of coal and especially to address problems associated with strip mining. The NCPP, brainchild of Gerald Decker of Dow Chemical, was extraordinarily successful in forging agreement among the potentially contending policies and in writing recommendations that were more than compromises—being, in at least some cases, innovative solutions. But the proposals for the most part failed to attract policy attention, for two basic reasons. First, the participants in the NCPP failed to involve governmental actors, either from relevant agencies or from Congress. Moreover, a window of opportunity did not exist for the recommendations, because of the passage of the Surface Mining Control and Reclamation Act of 1977 (viewed by most as a victory for environmentalists). The passage of this act did not resolve the matter, because many issues of implementation remained. But its passage, after extended controversy, did move the issue off the congressional agenda and made policymakers less attentive to the proposals coming from the NCPP.

More importantly, the proposals failed because the negotiators were unable to convince the organizations they represented to agree to the solutions in the report of the NCPP, entitled *Where We Agree*. The failure was particularly acute for environmental groups. Leaders from the Sierra Club had taken the lead in negotiations in the NCPP, but they were unable to convince the club board to support the measure. Once Sierra had opted out, no other environmental group wanted to take the lead and be accused of selling out. McFarland points to an interesting dynamic in public interest groups: they are unwilling to lose substantial membership (McFarland suggests a 20–25% figure) by endorsing a controversial, highly symbolic policy even though the vast majority of members are either supportive or indifferent. That leaves such groups hostage to extremists. Business interests were afflicted by some of the same problems (especially the recalcitrance of the coal-mine owners, in contrast to the more moderate stances of the major utility and chemical companies). But businesses firms are better able to move independently of industry associations.

Most of the book concerns just how the negotiators came to cooperate so productively. The key was the adoption of a set of negotiation principles developed by law professor Milton Wessel and going under the rubric of the "Rule of Reason." Although developed independently, these principles paralleled similar notions stemming from social-scientific study of negotiation and dispute resolution. Basically, the rules stressed respect for an opponent's position and the search for common interests. Most importantly, all participants began to appreciate both the need to pay attention to economic considerations and the need to protect the environment. A search for the facts and their relationship to a common framework replaced the advocacy approach that had characterized the relations among the parties. One of the most positive developments in the process was a major trade-off, in which the environmentalists were willing to move from a command-and-control model of environmental protection to an economic incentive approach that set public goals and allowed companies to pursue them through a variety of means, whereas the industry representatives recognized the desirability of public funding of public interest groups to monitor the process. (Remember that this was 1977–78, at the height of rancor between business and environmental groups.)

In this particular case, then, not only was the collaboration of producer and countervailing groups not a public policy nightmare, but it actually yielded enlightened proposals based on the search for common interests. This *cooperative pluralism,* although it failed in all but a couple of regulatory suggestions, nevertheless has emerged in a somewhat different form, *regulatory negotiation.* McFarland reviews several recent cases of regulatory negotiation and suggests the factors that promote success. Not surprisingly, perhaps the major criterion for success is the participation of the state in the form of a federal regulatory agency. The agency seems to play two roles: to encourage the sides to negotiate over the issue (which is generally narrower than the range of issues discussed by the NCPP) and to make the settlement stick by giving it the rule of law.

Cooperative Pluralism is a well-written, well-argued book with clear lessons for a critical issue of politics: What accounts for the patterns of division and cooperation among citizens and the groups that represent them? In the end, McFarland concludes that the narrowness of strategies pursued by groups and their contact almost exclusively with allies (as Heinz, Laumann, and Salisbury have shown empirically) limits the ability of the interest-group system to search for common-ground. What is wrong with pluralism, then, is its inability to promote communication across interests; that is, in a sense, it fails to give enough power over to the groups to forge policy solutions. I may be pushing McFarland a little further than he would be willing to go, but I would note the difference between his analysis and the in-vogue attacks on the interest-group structure represented by journalist Jonathan Raup's *Demosclerosis,* essentially a rehashing of Mancur Olsen's *Decline of Nations.* Group-hating Raup and Olsen make few positive suggestions about improvement. McFarland's work suggests a far more positive line of reforms, some of which are being put into place (such as regulatory negotiation). I would like to make this book required reading for all the special-interest bashers and gridlock decriers, but I realize that they would rather live in their own cynical world than think more clearly about what would improve the situation.

Texas A&M University BRYAN D. JONES

The President and the Parties: The Transformation of the American Party System since the New Deal. By Sidney M. Milkis. New York: Oxford University Press, 1993. 404p. $49.95 cloth, $19.95 paper.

Before the New Deal, American presidents were both empowered and constrained by their relationship to political parties, but modern presidents broke that tie. The modern presidency's increased autonomy from parties has been addressed in one fashion or another by important scholarship in the last several decades. Yet there is in the literature only partial or speculative explanations for the transformation from a party-based presidency to an institutionally autonomous presidency. In this excellent book, Sidney M. Milkis constructs a theoretically subtle and empirically rich account of that institutional transformation within the American regime.

Milkis treats the relationship of presidents and parties from Franklin Roosevelt to the present. For Milkis, the relationship of presidency to party is a lens for clarifying fundamental changes over time in American politics. In his analysis, the key development, triggering a complex system of changes, was the incorporation of a substantive policy mission into the institutional identity of the presidency. That change alienated the presidency from the parties and Congress and launched presidents into a search for an autonomous political base. In turn, Milkis argues, that search led to the creation of robust political resources for the presidency within the executive branch.

The vision of a reformist presidency was not new to the 1930s. The Progressive Era envisioned the president as a policy innovator, as evidenced by the president's changing rhetorical practice in that period. However, progressivism's visions of reform governance far exceeded the capacities for innovation and leadership of a party-dominated national government. To illustrate, Theodore Roosevelt was constrained by a Congress in which Republican "standpatters" dominated, and Woodrow Wilson could lead only so long as the congressional Democrats supported his initiatives. Indeed, more than Milkis recognizes here, the relationship between expanded administration and presidential power occurred to the Progressives, but their attempts at building increased presidential capacity were stymied by the parties and Congress.

It was the New Deal that fully associated the presidency with programmatic liberalism. Like his Progressive predecessors, Franklin Roosevelt ran up against the barrier of localist and fragmented political parties. So powerful was the reaction against Roosevelt in parts of the Democratic party by the late 1930s that conservatism overcame partisanship in the formation of the congressional coalition of Southern Democrats and Republicans.

Although Franklin Roosevelt won the 1932 presidential election in a realigning party victory, the Democratic party was not a consistent vehicle for programmatic liberalism. Roosevelt fitfully attempted to change the Democratic party, but his central effort was to fashion a base for executive power in the administrative state itself, separate from parties and Congress. Roosevelt created what Milkis terms a new, administrative constitution. Building on the work of New Deal historians, such as Barry Karl, Richard Polenberg, and Otis Graham, Milkis's analysis deepens our understanding of the so-called Third New Deal. He portrays Roosevelt as reconstituting presidential authority. While FDR was apparently beaten on several fronts in the late 1930s, Milkis convincingly argues that Roosevelt's strategy succeeded in crucial respects. The president lost the court-packing fight and failed to get his reorganization bill in 1937 and 1938. However, by 1939, the Supreme Court had switched sides. The president also got a reorganization bill that gave him major new authority, although less than had been recommended by the Brownlow Committee. Equally important, Roosevelt succeeded in the alchemy of turning a key New Deal policy—the 1935 Social Security Act—into a personal right, protected from later politicians and parties.

Post–New Deal presidents were inheritors of the autonomous presidency that Roosevelt founded, and parties themselves receded in importance in the 1950s. Voting turnout declined after the New Deal realignment, which itself evidenced less intensity of electoral participation than earlier realignments. Additionally, presidential selection was increasingly personalistic, more dependent on individual candidate organizations than upon central party decision making. The advent of televised politics and the importance of primaries heightened the independence of presidents from parties. Thus, parties seem to fade from center stage in American politics.

Milkis warns that weakened parties and autonomous presidents have highly undesirable consequences for democracy. Expanded administration gave presidents more capacity and independence. Yet, as Milkis argues, presidents became politically isolated, dependent on their own organizational apparatus and their fleeting public appeal. Their incentives were to make their policy preferences safe from politics by converting government benefits to rights. The creation of public policies as individual rights, or entitlements, in turn, stifles the possibility of continuing public deliberation and debate about national priorities.

From Truman to Johnson, presidents found in the remaining liberal agenda and (one might add) the Cold War justifications for exercising power independent of party support. However, through his call for more participatory and less bureaucratic government and through his conduct of the national-security state, Johnson undermined the administrative constitution's legitimacy.

How did the administrative constitution fare with Republican presidents who were unencumbered by programmatic liberalism? Milkis shows that they also embraced the administrative constitution but for different ends. Facing hostile majorities in Congress, Republican presidents took the administrative constitution as a means for presidential leverage against the liberal policy establishment in a divided government. As Republican presidents turned the administrative constitution towards the end of redirecting government, they transformed its purposes. Milkis observes that with Richard Nixon it became clear for the first time that "the modern presidency could be characterized as a two-edged sword, which could cut in a conservative as well as a liberal direction" (p. 225). Yet even as Republican presidents embraced the administrative constitution, its legitimacy was sinking. Given divided government and in the wake of the collapse of the liberal agenda that undergirded the administrative constitution, presidential claims to power through it were received skeptically by Congress and the public, triggering new interest group and congressional activism to constrain presidents. Consequently, presidents were tempted to misuse their administrative capacities. Covert uses of executive power might shield the incumbent from public distrust of the administrative constitution but to would weaken the presidency over time.

Has the period since the New Deal, and the administrative constitution, permanently divided the presidency and the parties? Are there any conditions under which presidents and party might share a conception of governance? Milkis discusses the turn toward programmatic conservatism in the Republican party, but he does not consider the consequences of that development for an undivided Republican government, should one occur. It is conceivable that programmatic conservatism might unite what programmatic liberalism divided in American national politics. In the context of undivided government, Republican politics might produce something more akin to a Wilsonian presidential-party government than to the model of presidential autonomy that has formed the executive since the New Deal.

Whatever relations obtain for the presidency, the parties, and Congress in the next decade, *The President and the Parties* will be an important guide to understanding these institutional dynamics. Milkis has accomplished a superb marriage of political history and social science explanation, and his book offers a fundamental explanation of the institutional politics of the modern presidency.

University of Notre Dame PERI E. ARNOLD

Corporate Political Agency: The Construction of Competition in Public Affairs. Edited by Barry M. Mitnick. Newbury Park: Sage, 1993. 327p. $46.00 cloth, $23.95 paper.

Mitnick's edited volume of 11 essays outlines a theory in which political activity by corporations is an important and legitimate part of democratic competition. There are two key concepts that pervade the work in the volume: the analytic concept of *agency theory,* which Mitnick helped create, and the normative value of *contestability,* used to judge the quality of democratic competition. While the notion of contestability is not quite as powerful (or as novel) as Mitnick would have us believe, this book sets a new standard in combining current research on practical corporate strategy with normative political science.

Democratic competition is conceived in two very different ways in the literature: (1) as a means for discovering truth and (2) simply as a way in which groups choose. To discover truth, the key component of democracy is open debate, with free access to the means of persuading others. In a New England town meeting, each citizen might be allotted an equal amount of time to speak, but some speakers are more persuasive in both their ideas and their rhetoric. Citizens might willingly yield their time to the articulate few they agree with, who can make better use of the scarce resource. Citizens might contribute money or labor to build a platform so others can see or buy an amplification system so others can be heard. As Walzer in *Spheres of Influence* (1983) and Graber in *Transforming Free Speech* (1991) have noted, the contribution of valuable resources may actually help illuminate the best choice.

On the other hand, if democracy is just a choice, any translation of economic power into political power is a definitional deformation of democracy. Money is spent on amplification not just to reach all citizens but to drown out competing (perhaps more persuasive) voices. Platforms are raised not just high enough so that voters can see but tall and bauble-encrusted enough to attract the eye and dull the mind.

Forty years of social choice theory have lent credence to the "democracy reveals a choice, not the truth" position; yet the U.S. Supreme Court has held (most notably in *Buckley* v. *Valeo* of 1973) that in many cases *money*, even corporate money, is protected speech. The contributors to the volume pay lip service to the notion that democracy is just the way people choose and note that no overall preferences or goals can be attributed to the organic society that makes the choice. Nonetheless, Mitnick's proposed test of the quality of a democratic system reveals a solicitude for truth-value of the outcome: "The test of a democracy is not if its rules seem fair, but that its system is contestable" (p. 21). *Political contestability* is defined as "the extent to which a collective political actor or a system of such political actors possesses attributes, resources, positions, or other factors, in themselves or in their environments that promote the ability to compete effectively" (p. 12).

If this is the definition of a "good" democratic system, what are the policy implications? Contestability must either be an outcome-oriented criterion or else focus on exactly the fairness of the rules of competition. Mitnick does offer a criterion for judging outcomes by emphasizing agency theory, or the design of a contract that induces goals in an agent coincident with the preferences of a principal. But since Mitnick has recognized the social choice problems inherent in identifying a Rousseauian principal, there is no standard against which to compare outcomes.

On the other hand, if forced to fall back on the rules of process that dictate competition, Mitnick and his contributors rely on fairness, which violates the book's own canon for testing democracy. Mitnick invokes Rawls's *A Theory of Justice* (1971), noting: "Such reasoning leads to support for built-in protections for individual participants against persistent system bias. In essence, *mechanisms are needed to keep the playing field fair and level*" (p. 13, my emphasis). At best, Mitnick's definition of a good democratic system appears to rest on the distinction between rules that in and of themselves *seem* fair based on some ethically derived notion of justice and rules that really *are* fair, because it is possible for anyone with a persuasive argument (presumably, the truth) to win.

One wishes there were a recognition of the intellectual linkages between this position and more traditional political theory, particularly since Mitnick and his contributors are concerned with the political actions of corporations. Mitnick does recognize the difficulty, but relies much more on theories of the corporation (e.g., Epstein's *The Corporation in American Politics*, (1969) than on political theory. To be fair, Mitnick's extensive review of Epstein's famous book is revealing; and political theorists would find the arguments Mitnick derives from it to be powerful. Updating Epstein's thought and extending the analogy of economic contestability to politics, with the obvious implications for entry barriers and the sunk costs of reputation, is a valuable enterprise and one that Mitnick accomplishes well in his opening chapter. Though Mitnick's apparent aspiration to lay out "the core of what could be an interesting, explicit, generalized theory of competitive constraint" (p. 36) is not fully realized here, the argument he makes is an interesting and useful one. The perspective this volume offers is a very nice mix of the practical and the prescriptive: "We acknowledge that the playing field is tilted. We see elephants [corporations] among the chickens [voters]. But instead of throwing up our theoretical hands, we simply elect to call for better rules of the game for our playing field, but just the minimal rules necessary to make it level and give the chickens (and any other barnyard participants) an even chance" (p. 61). The mixture of the playing field and barnyard metaphors appears intentional. Perhaps Mitnick is suggesting that in dealing with the corporation in modern democracy, we should continue the game but watch where we step.

University of North Carolina, Chapel Hill MICHAEL C. MUNGER

The Learning of Liberty: The Educational Ideas of the American Founders. By Lorraine Smith Pangle and Thomas L. Pangle. Lawrence: University Press of Kansas, 1993. 350p. $35.00.

This very important book is original, sweeping, and wise about the relation between education and liberal democracy in the United States. The Pangles have not written a history of education or an account of present-day policies, but they reconsider superior ideas from the founding period in a way that illuminates any serious thinking on American education, whether policy-oriented or historical. That is their intent. Americans, they suggest, have become "uncertain" as to "what the proper goals of education are" whereas the founding generation had to think through afresh such fundamentals (p. 6). While American schools have turned to dwell on useful skills, openness to other cultures, and amelioration of social problems, there is widespread concern about a certain emptiness as to "distinctly American, republican principles" and as to a moral education that might help the young "go beyond fleeting amusements to find lasting and meaningful happiness" in life (p. 7). There is even a widespread skepticism about whether such principles and aims can be known. To help in our uncertainties, even in reconsidering what is a "truly educated and well-formed human being" (p. 285), the Pangles turn to resurrect the educational proposals and reflections of Benjamin Franklin, Thomas Jefferson, George Washington, Noah Webster, Emma Willard, and many others. But the spirit is of reassessment, not of hero worship. They would "reanimate" the "living thought" of only those among the founders who spoke "most compellingly and clearly" on the topic, and they would subject it to "critical analysis and dialogue" (p. 6). The book is a mixture of dramatic American views with sympathetic but continuous interrogation, occasionally seasoned with the reflections of a Milton, Locke, Aristotle, or whoever.

The gist of the story is that education for a liberal order was problematic from the start, and the founders faced the problem. The modern Lockean tradition was not fully adequate to a democratic society, and the premodern educational traditions of classical republicanism and Christianity were thought largely irrelevant. Also, some of the founders' plans had their own difficulties, and a stingy and divided public was reluctant to fund or accept projects however impressive. But the big story is the founders' resourcefulness. The Pangles discuss (1) plans for schools, textbooks, universities, philosophic societies, libraries, and almanacs; (2) efforts to shape religion, the press, politics, and the economy with a view to civic education; and (3) proposals for civic models to live by, such as the great Washington or the self-made man of Franklin's *Autobiography*.

The book displays a startling array of proposals and reflections, especially as to moral and civil education; and this is its most obvious achievement. There are fascinating accounts of Franklin's paradigmatic plan for a Pennsylvania academy, Webster's best-selling spellers and readers (laced with civics, including a "Federal Catechism"), Emma Willard's female seminary and her then-radical plan for training female teachers, and of course Jefferson's paternal plans for a whole system of schools and the University of Virginia.

Amid the particulars, the authors clarify the importance and special difficulties of civic education for a liberal order, and this is a greater achievement. Statesmen like Jefferson and John Adams saw that American education could not be aimed at fostering Protestant zeal or Anglican tepidness; the manners of a chivalric aristocracy; or the strict, patriotic, and religious traditions of a Sparta or an Athens. Liberal democratic citizens were to be autonomous and humane—both liberated from reverence for tradition and enlightened as to the economic and governmental means of progress. But this modern outlook provoked its own problems. In general, the priority of civic education conflicted with the priorities of rights and of democracy. It is hard to evoke reverence for law when law, even constitutional law, is but instrumental to one's rights. It is difficult to foster citizen education by political participation, as Jefferson wished, if one would reduce the purview of government and thus remove incentives for participation, especially by more ambitious citizens. It is hard to balance popular vigilance regarding government and ambition with respect for government and for honorable patriots, such as a Washington. Jefferson raised the now famous wall of separation between church and state. But he also questioned whether belief in the rights of man could survive without their "only firm basis, a conviction in the minds of the people that these liberties are of the gift of God" (p. 193), and he recast Christianity into a humane and civil creed. The Pangles look for prudent "balance." In an enlightened republic, for example, concern for the press's rights should be accompanied by concern for its "mission." While this mission is partly to act as watchdog guarding against governmental abuses, it is also and chiefly to act as an "auxiliary" of government in civic and moral education and an auxiliary also of the "learned" in making publicly appealing what in their thought is publicly useful (p. 223).

The highpoint of the book is its concluding discussion of education by models of emulation. The authors appreciate the moral heights of a Washington and the models of the common man set forth by Franklin and Jefferson. They are philosophically precise as to qualities and politically grave as to import. Washington was "an object of awe for all the founders" (p. 234). Many held him up, as he actually held and governed himself, as an inspiring personal example for military officers, future presidents, and in general the young who are potential leaders. The authors commend such examples. Perhaps the very core of moral education should be "the study of rich biographies of individuals who exemplify different or competing versions of human excellence" (p. 287). Besides, sour-eyed cynicism corrodes respect for law, government, and leaders. The kind of model that Washington, in particular, offers "is what is most needed in the civic education of any republic, and especially that of contemporary America" (p. 248). The authors question scholarly reductions of the Washington's stern virtue to love of fame or to a need for public approval. They draw finally on Aristotle's description of the great-souled man. While they review sympathetically Jefferson's moral common man, they find grave difficulties in the crucial doctrine of the "moral sense." It is Franklin who thought out most deliberately and adequately a model common man who can mix independence with public service. Franklin is the "contested genius of American education" (p. 278), and the Pangles' clarification of the genius informing the *Autobiography* is superior to any other account known to me. Their tale is largely of greatness in the guise of humility. They bring out Franklin's mixture of self-interest, public-spiritedness, and devotion to science; the immense talents at story, ironic self-deprecation, and other arts of popular rhetoric; and, in general, the penetration and sobriety that can produce a moral teaching for the millions that also finds places for the superior. Yet while mostly rejecting the depreciations of Franklin by Max Weber and D. H. Lawrence, they conclude that Lawrence was partly right. Franklin's slyly comprehensive rationality lacks an appreciation of the soul's deeper longings for nobility and clarity and for a transcendence unbounded by human limits.

The Pangles think that general civic education is most obviously important (and especially slighted in our time) but not sufficient for judges, legislators, and other leaders. Nor, they believe, is civic education, common or uncommon, adequate for those drawn to literature, art, or philosophy. These thoughts lead them to clarify the various kinds of education in their American forms and places, the conflicts between them, and the priority and fragility of the most intellectual kind. Such clarifications are the book's highest and most probing achievement. The authors' appreciation of the University of Virginia, for example, concludes that Jefferson's curriculum was intellectually shallow partly because it was too civic. Jefferson gave short shrift to foreign languages and shorter shrift to literature (notably modern literature), dwelling instead on the natural sciences and government. He also excluded political texts alien to a states' rights republicanism, which led the university to become a school for Southerners and the Southern cause and excluded any "premodern or pre-Lockean political theory" (p. 173), especially ancient philosophy and other accounts of ancient republicanism. Jefferson was right to put civic education as a first and last duty of professors of government, the Pangles conclude, but he made civic education too partisan and curtailed philosophic education. He thus left students "rooted in thin soil" and lacking experience of the "zest for intellectual debate, and a consequent joy in learning and reading" that come with confronting "fundamental alternatives" (pp. 174–75).

It is striking that a book so filled with controversial judgments about education for politics and the politics of education is not "politicized" in the current sense. The book seems studiedly undoctrinaire, as if the authors intend to present a model of open and direct inquiry for those seeking a thoughtful yet imaginative path amid the fortified ideological camps of the present. They would educate for liberal liberty, but they are aware that this is a problem and are also aware of other plausible regimes and educations. They seek out some of the best proposals for American circumstances, quietly showing the self-contradiction and irrelevance of a generalized relativism; but they confront the diversity of proposals openly and take arguments for and against seriously. They show why a thoughtful citizen should take a certain stand on behalf of a liberal order, an informed and law-abiding citizenry, and honorable statesmen; but they also show the need for the impressive thoughtfulness of a Jefferson or Franklin and for a Socratic thoughtfulness that regards—seriously but not reverentially—impressive thoughts on politics and education. This book is a model of how to mix practical seriousness about American politics with philosophic seriousness about thinking things through. The style reflects the content. There are 14 chapters, any one of which contains more digested thinking about important American matters than most books; yet the whole text is only 288 pages. Pedantry and complacency are absent, and the reader is drawn into constant consideration and questioning. The Pangles are closer to educationalized politics than to politicized education; but a warm respect for the advantages of a liberal country and a cool awareness of its insistences and necessities keep them also from that extreme.

In a book so packed with controversial thought on controversial topics, there will be plenty of nits perceived and the occasional nit to pick. I wonder whether John Locke is not a bit tougher regarding the disciplining of children and a bit more radical in revolutionary indoctrination than the Pangles say and whether Francis Bacon is enamored of literature for its own sake; and it would have been useful to have had more comparisons with the key educational developments after the founding era. But as far as I can see, the Pangles have the big picture right (and often righter than any other scholarly picture) in every one of their 14 big discussions. To concentrate on nits would miss a superior rethinking of some excellent American political–educational formulations, in which they become again inspiring and instructive in our rather disheartened and confused time.

Boston College ROBERT K. FAULKNER

Federal Policymaking and the Poor: National Goals, Local Choices, and Distributional Outcomes. By Michael J. Rich. Princeton: Princeton University Press, 1993. 432p. $39.50.

For policymakers seeking to concentrate resources on the poor, the American political system presents a formidable challenge: federalism offers multiple arenas for arguing on behalf of the poor, yet creates numerous incentives and opportunities for diverting resources and spreading them more widely. Michael J. Rich takes up this central dilemma of American social policy in a multifaceted, empirically rigorous analysis of the Community Development Block Grant program (CDBG).

Rich does a excellent job of capturing the complexity of American policymaking in an analysis that stretches across levels of government and over time. He traces the development of the CDBG program at the federal level from its inception in 1974, showing how the distributional consequences of CDBG grants varied across administrations and noting the ongoing conflicts between Congress and the Department of Housing and Urban Development over how targeted the program should be. His study highlights the importance of examining implementation in the American context: as his cases show well, state and local decisions determine the ultimate impact of federal programs. Rich uses several different approaches to explore the effects of state and local decisions. He examines the influence of states using data from the small-cities program to show how states distributed resources. Local effects are examined in case studies of Chicago, several suburban cities, and two urban counties in the Chicago area. Rich analyzes data from these programs to show the patterns of targeting to poor neighborhoods in these diverse jurisdictions.

The analysis of the state-level programs is particularly valuable. Because states were given the option of controlling the small-cities program in 1982, Rich was able to compare the period of federal control with that of state control to test a long-standing question in American politics: Which level of government best serves the poor? His analysis demonstrates that while some states targeted more than the federal government, on the whole the federal government was more likely to direct funds to distressed communities. In addition, he provides a very interesting and well-executed analysis of the kinds of state institutional arrangements and political conditions that facilitate targeting to needy places. He shows that states with unified political control of the executive and legislative branches and little political competition achieved the greatest levels of targeting. The analysis also reveals that states that had designed targeted programs in the past were more likely to target CDBG funds (although where that initial propensity to target comes from remains unclear).

The case studies of the Chicago metropolitan area highlight the variability of local decisions. The Chicago analysis shows that mayors can make a difference in how federal funds are spent. Harold Washington, Chicago's first black mayor, targeted funds to needy neighborhoods more than did his white predecessors and relied on community-based organizations more than did previous mayors. Unfortunately, Rich could not fully consider the effects of the return to a white mayor in 1989, since data were available for only the first two years of the new Daley administration. Rich's analysis of the suburban cities and urban counties is especially useful because we know much less about suburban politics than city politics. His data show that targeting varied widely in the suburbs. But perhaps most importantly, he demonstrates that even when suburbs did target, they focused on well-organized constituencies, such as the elderly and the disabled, rather than the poor. Rich restricts his analysis to six very affluent suburban cities. It would have been interesting to compare the distributional patterns in less-well-off suburbs as well, since the politics of these changing suburban areas remains largely uncharted territory.

Rich is at his best in nailing down the distributional consequences of policy. This book is full of careful, detailed empirical analyses clearly presented and effectively displayed. However, its treatment of the politics underlying these patterns is less fully elaborated. Rich uses the notion of *benefits coalitions* to discuss the political prerequisites for targeting, but we learn little about the conditions under which benefits coalitions in favor of targeting are likely to emerge and become successful. The discussion of benefits coalitions is not systematically linked to the analysis of the institutional features of decision-making systems; the appearance of these coalitions is essentially ad hoc.

One major policy implication flows quite clearly from Rich's analysis. Rich cogently argues that if our goal is to channel resources to the needy, a stronger federal role will be needed. Although federal pressure to target does not guarantee that money will be spent on the poor, it strengthens local coalitions that favor targeting and allows local officials to deflect pressures to distribute funds more widely. However, Rich slides over the important question whether it is better to use federal funds to aid people or places. He simply asserts that it is important to aid poor places, in addition to poor people. Because the CDBG program has proven to be such a "leaky bucket," it would be useful to know more about why we should continue place-targeting. Or, given the difficulties of targeting under block grants, wouldn't it make sense simply to return to the categorical grants of the 1960s?

These caveats aside, *Policymaking for the Poor* does an excellent job at its main task, which is to show how decisions at different levels of government affected the distribution of CDBG funds. Targeting remains a critical issue in American social and economic policy. Rich's work suggests that the federal government is now less likely to target funds to needy areas. As he shows, the population shift to the suburbs and the decline in urban political power have increased the pressures to spread resources more widely. And as President Clinton found out in 1993 when Congress defeated his request for a major increase in CDBG funding, charges of pork-barrel spending pose new political dangers in a climate of budgetary stringency. Rich's empirically rich and careful analysis illuminates just what is at stake in such decisions and offers us a set of tools for tracing their repercussions across the federal system.

Brookings Institution MARGARET WEIR

Congress, the President, and Policymaking: A Historical Analysis. By Jean Reith Schroedel. Armonk: M.E. Sharpe, 1994. 240p. $55.00 cloth, $22.95 paper.

The study of presidential-congressional relations has undergone something of a renaissance in recent years, marked by a spate of books and reinvigorated arguments over the proper relationship between these two branches of government. This renewal of interest in a very old subject has been spawned by both an empirical and a normative concern: the empirical derives from dissatisfaction with the limitations of existing paradigms, and the normative from both an academic and a political debate (assuming a real distinction between the two) that variously wrings its hands over the impotence of modern presidents or the imperial excesses of intrusive congresses.

While some analysts have done a poor job at concealing their narrow partisan interests, the debate has for the most part been enlightened and enlightening. Into this fray steps Jean Schroedel, who seeks to tackle several particular aspects of the larger analytic and normative debate. She notes correctly several key problems, including the historical nature of much modern presidential-congressional analysis, its presidency-centrism, and its neglect of constitutional context. Schroedel's primary argument seeks to advance a notion of "dynamic constitutionalism," which attempts to "recognize the Constitution as a major determinant of legislative behavior" (p. 6). Quite simply, this translates into an acknowledgment that the Constitution sets the boundaries, parameters, and the broader political/institutional culture in which the governing institutions operate.

Schroedel applies this principle in both quantitative and qualitative analysis of all banking legislation (whether enacted or not) with which Congress dealt between 1823 and 1986. Her primary quantitative data set is 6578 banking bills spanning this period; her qualitative analysis focuses on three case studies. Although she carefully justifies her selection of banking regulation policy, Schroedel fails to note that the work of Lowi, Tatalovich and Daynes, and others has found that, at least in the modern era, presidents have exercised the least degree of political influence over regulatory matters in Congress, meaning that this policy area almost certainly represents the nadir of presidential influence in the domestic realm. Nevertheless, the long historical span does allow for viable longitudinal analysis, and her emphasis on analysis of all bills, rather than merely those enacted or those emphasized by presidents, sets this study apart from most others on the subject.

Complementing the historical perspective, Schroedel also scrutinizes vital elements of the legislative process too often ignored by those whose analysis begins and ends with roll calls, including the pivotal role of congressional committees and committee chairs, consideration of the full range of bills from minor to major, and careful scrutiny of decision making on the floors of the House and Senate, leading up to and including final votes. Schroedel's full view of the legislative process leads her to conclude that the overwhelming majority of bills (over 90%) proceed untouched by presidential influence. She does observe, and therefore confirms, familiar historical patterns of rising presidential influence and involvement, especially around the turn of the century and since the New Deal era. Still, she helps to deflate some of the exaggerated claims of presidential power that are too frequently repeated, and reassert the truth of James Madison's dictum that the legislative branch is still indeed the first branch of government—at least when it comes to the full array of legislative affairs. She also offers some insightful criticisms of such current theories as Peterson's tandem-institutions perspective, and Skowronek's political time view. Her effort to empirically verify Skowronek's perspective actually does more to buttress Walter Dean Burnham's parent theory of realignments, upon which Skowronek's work rests.

Schroedel's return to constitutionalism is welcome, but also ironic, because her use of the constitutional perspective is contextual, not textual. That is, her constitutional analysis is narrowly operationalized as political influence over congressional politics, as shaped for example by constitutionally mandated term lengths and differing constituencies of the president versus members of Congress. Missing is any inter-branch analysis based on statutory law, constitutional law, or administrative law. I do not seek to fault her analysis for failing to examine these matters (which is well done by such scholars as Louis Fisher), but to note that an approach claiming to be "constitutional" can hardly carry such a label absent these or similar considerations.

The most engaging portion of the book is the three case studies. Yet Schroedel fails to use these cases to best advantage. The cases are described with great care, yet barely two pages are devoted to a synthesis of the cases, and their lessons seem to contribute little to the larger analysis. In addition, the three cases—creation of the postal savings bank (1873–1910), the depositors' guarantee fund (1886–1933), and the federal reserve act (1886–1913)—all span roughly the same time periods, if one considers the starting point the initial introduction of the bill, and the end point as the program's final passage. The empirical analysis could also benefit from more extended discussions of findings, significance, and implications.

Schroedel's analysis also underemphasizes the extent to which presidents take an interest in parochial political matters, especially as they pertain to the president's own re-election concerns, and presidential efforts to influence members of Congress. Similarly, she underestimates Congress's ability to grapple successfully with broader national and international policy concerns. The four central assertions Schroedel offers at the outset of the book as "testable propositions" are hardly that. For example, the "proposition" that "Presidents will need the support of key congressional actors if they are to see their initiatives enacted" (p. 12) is both self-evident and axiomatic. At best, these are assumptions about where analysis begins.

The centerpieces of Schroedel's analysis—its historical bent, and its sweeping look at all legislation within a policy category—represent an admirable and useful approach worthy of replication by other scholars. The prudently-imposed limitations of the study increase analytic feasibility, but decrease the study's generalizability, and her conclusions stretch well beyond her data base and chosen policy area. The empirical analysis is intriguing, but not conclusive. In sum, Schroedel has widened some important new avenues of exploration, but has left existing questions and controversies pretty much where they were.

SUNY, Cortland ROBERT J. SPITZER

Burying Uncertainty: Risk and the Case against Geological Disposal of Nuclear Waste. By K. S. Shrader-Frechette. Berkeley: University of California Press, 1993. 346p. $40.00 cloth, $15.00 paper.

If the technological capabilities of humanity have outstripped our moral and political capacities to control and create our collective future, part of the problem may result from the fact that more research time and money is committed to the empirical study of environmental policy than to the normative ramifications of science, politics, and public administration. Most of the published literature relies on the axioms of economics and/or physical science as the "certain" foundation of policy analysis. But if our future requires sound and comprehensive collective choice that takes the normative dimensions of decisions seriously, then we must hope that there are scholars out there willing to push the envelope of empirical presumption—scholars, that is, like K. S. Shrader-Frechette.

This is a unique and exceptional book on many levels. It is a consummate case study of the Yucca Mountain High-Level Nuclear Waste Depository siting that not only criticizes current policy but suggests an alternative course of action. It is a detailed and revealing analysis of scientific assumptions and their implications in considering the geological disposal of nuclear waste. It is a fascinating study of the political and administrative strategies that have produced our current approach to environmental law. It is an ethical argument about the affects of science policy decisions on democratic government. It is a complete and timely examination of quantitative risk assessment. Lastly (but most importantly), it is a comprehensive normative study of an environmental risk policy that not only makes ethical arguments about nuclear waste disposal but also points out the normative character of the epistemological and methodological assumptions that science brings to policy decision making.

Environmental policy inevitably involves science. Anyone approaching a public choice regarding humanity and nature must consider the physical and biological ramifications involved. They are rightly assumed to be a critical component of proper policymaking. However, there is much written on the utility of scientific data and the question of policy as a "transscientific" phenomenon; and when one is operating within the uncertainty that is environmental risk, these questions become more critical to both *which* scientific assumptions are given credibility and *how* these are used in the formulation and analysis of policy alternatives.

If imitation is the highest form of flattery, the scientific community has been envied by many in the humanities and social sciences for the "factuality" and "certainty" of its "empirical" subject-matter. For decades, quantitative risk assessment has provided the science in the formulation of environmental risk policy and, for the most part, has been either accepted or rejected whole-cloth, by the public administrator or analyst, without a detailed inquiry into its underlying assumptions.

Throughout her book, Schrader-Frechette alternates between a general analysis of the presumptions of quantitative risk assessment and a specific criticism of the proposed Yucca Mountain High-Level Nuclear Waste Depository. The author contends, persuasively, however, that the entire policy argument for Yucca Mountain, including its science, is grounded in normative presumptions and arguments; and during this guided tour, one is amazed at both the narrowness of current thinking and the number of alternative assumptions and improvements in scientific inference that are accessible to the analyst in synthesizing a more complete policy argument.

The major focus of the book is, therefore, ethical and scientific value judgments. In terms of the normative values of science, the author takes many angles of criticism (priority of false positives or negatives, use of worst-case scenarios, de minimis inferences, linearity, begging the question, two-valued epistemic logic, the appeal to ignorance, acceptability of current magnitudes of risk, etc.) and makes her case in simple, concise and inherently logical terms. In addition, the author's discussion of moral values (e.g., intergenerational justice, consent, and the general reliance on preference based utilitarian moral principles when noninstrumental, nonpreference values are at stake) is enlightening and shows that both types of values share degrees of "subjectivity," "objectivity," and uncertainty, as well as inherent points of contention that must be debated within the context of fully integrated arguments. Together, the combination of these two avenues of normative criticism convince the reader that "facts" and "values" are both creatures of persuasive argument that (as demonstrated in the case study contained within this book), united in the right order and combination, can open doors to the comprehensive understanding of a policy issue—doors that were previously assumed to be either nonexistent or already shut.

In addition to its critical argument concerning Yucca Mountain, the book also contains an interesting constructive argument. The author contends that "negotiated, monitored, retrievable storage (NMRS) facilities" will "not misrepresent our scientific uncertainties or misappropriate our ethical burdens" (pp. 215, 251) and are preferable to permanent geological storage like that planned for Yucca Mountain. Although only one chapter is devoted to this constructive argument, it is adequate, given the critical argument that preceded it, to make the author's point. By the time one is finished with this book, its prescription seems eminently reasonable.

Overall, this book contains a complete and unique policy argument. It gives insight into the scientific assumptions at the foundation of current policy decision making and goes further to recommend a solution built upon the criticisms of current policy analysis. More than anything else, however, this book reveals the normative character of scientific inquiry and is an indispensable guide to the uses, abuses, failures, and potential of science in environmental policy decision making. If it did nothing else, it would be a seminal case study in the field of nuclear waste policy analysis and administration; but it does so much more.

University of Hartford and Vermont Law School — JOHN MARTIN GILLROY

The White House Speaks: Presidential Leadership as Persuasion. By Craig Allen Smith and Kathy B. Smith. Westport: Greenwood, 1994. 288p. $55.00.

The White House Speaks is another in Praeger's useful (but over-priced) Series in Political Communication. The series includes titles dealing with the media and public policy, the communication practices of citizen interest groups, the rhetoric of the cold war, presidential campaign films, journalistic ethics, and the modern campaign's emerging communication technologies (satellite dishes and the like). Many of the books in this series are genuinely thoughtful while others are not. Almost all, however, direct attention to the rather extraordinary intertwinings that have developed between governance and the communications industry. Some political scientists still cringe at the electronic Valhalla that has resulted, but with the first floor of the White House now devoted almost exclusively to communications staffers and with over 60% of all campaign expenditures now media-related, there seems little doubt that we need to know more about how political persuasion works.

The Smith and Smith book moves us down that road, but perhaps only a mile or two. In part that is a function of the book's genre—it is a collection of essays. Six of the nine chapters present individual case studies of the persuasive problems faced by the chief executive: Jerry Ford telling the VFW's faithful that he will pardon draft dodgers; George Bush winning a campaign via the rhetoric of divisiveness and then losing a presidency with the same approach (the snarling Pat Buchanan and all that); Jimmy Carter explaining to the American people why they should be willing to give up their canal in Panama; Harry Truman orchestrating a war-time rationing program for the nation; Ronald Reagan justifying why he behaved so unpardonably during Iran-Contra; and Richard Nixon explaining why he erred, pardonably as it turned out, during Watergate. Each of these stories, and a few others, is retold by Smith and Smith with enviable clarity and, often, with a deft critical touch. But there is no getting around the fact that their assemblage is an assemblage, and that it causes one to lurch a bit unsteadily when moving, say, from John Kennedy's civil rights rhetoric to Jimmy Carter's meditations on the nation's energy supplies. Each of these stories is interesting in its own right but one still comes away from *The White House Speaks* a bit dizzy.

The authors seem to have sensed that possibility and so present in chapter two their "interpretative systems approach" to matters political. Ostensibly that puts the book in theoretical perspective. Actually, the chapter does little more than lay out the various terms and concepts the authors use when probing the texts on which they focus. The result is more an interpretive orientation than true systematicity. For example, the authors describe how political logics differ from philosophical logics and how both differ from rhetorical logics. They explain how political stories work and why propositionality often takes a back seat in modern politics to narratives-well-wrought (as wrought, say, by a Ronald Reagan). And the authors explain how the nation is made up of a variety of "interpretive communities" that consume political messages in exceedingly curious ways, although it is not clear if such communities are politically unique or epistemologically unique (the distinction is a not unimportant one).

At various places in the book the authors nicely deploy these exegetical notions but the critical system *as a system* rarely functions. When that fact is combined with the volume's chain-of-beads structure (here Roosevelt, there Clinton), the book's bookishness dissolves considerably. This leaves the reader pining for some sort of over-arching argument.

Nevertheless, the book has its attractions. The authors write in an unpretentious manner, their case-studies are intelligently chosen, and their criticisms are functional (although they probably contain too many right-angles for the poststructural reader). Moreover, the authors make some interesting claims. They note, for example, that unlike Ronald Reagan, Bill Clinton has often become the centerpiece of others' narratives rather than the master of his own. The authors display the extraordinary *range* of rhetorical duties now imposed on the chief executive (priest, prophet and king, in Michael Novak's memorable phrase) and they describe the rhetorical, not just emotional, utilities of political passion—it enforces a coherent narrative (as, for example, during the Panama Canal debate). And the authors explain how presidents often paint themselves into political corners via their semantic choices, as did Jerry Ford when making himself a too-central figure during the absolving of Richard Nixon.

The White House Speaks will serve especially well for the new student of presidential discourse. The advanced student will need more. One especially important need is to determine what sorts of case studies should be done and why. From a theoretical perspective, not everything a president says deserves book-length treatment and not every political situation has rhetorical importance. We need to know better which studies of political persuasion should be undertaken and which abandoned. We need smarter reasons for doing critical work and fresher arguments after having done it. We need to tap political theory more aggressively, asking what a Rousseau or an Arendt or a Rawls might say about the new communications revolution (we have already heard from Foucault on the subject). We need to be less cavalier when positing simple effects from discursive acts and we need to reckon in more complex ways with the concatenations between rhetorical and non-rhetorical forces in political life. We need to acknowledge that presidents persuade, but we need to treat that as a premise and not as a mantra.

Right now, too many bad questions are being asked about political persuasion and too many simple answers given. We need to resist both conditions but we must also be bold enough to take on the dauntingly complex questions raised by mediated politics. It is easy to be silly about a great thing, especially a great new thing like modern communications. But it is even sillier to leave the matter unstudied.

University of Texas, Austin RODERICK P. HART

Media and Public Policy. Edited by Robert J. Spitzer. Westport: Praeger, 1993. 235p. $47.95.

The mass media have been blamed for everything from erratic policymaking to biased and unfair coverage. According to critiques by Polsby, Hess, and others, common patterns of media coverage create several problems for policymakers. Most problematic is the tendency of reporters to emphasize "the game" over the substantive issues of politics. Far more attention is devoted to personalities and specific events than to longer-term policy development. This creates a faddishness to reporting that makes it difficult for policymakers to devise clear, comprehensive, and coherent policies. Taken to its extreme, this interpretation places a substantial portion of the responsibility for policy inaction squarely at the feet of the media.

Yet despite the widespread acceptance of this critique, there has been surprisingly little systematic analysis of this relationship. This volume edited by Robert Spitzer provides an important corrective to that state of affairs. Based on papers delivered at recent conferences, the book features contributions from 15 scholars, who address the link between the media and public policy. It should be required reading for every researcher with a serious interest in public policymaking.

The primary contribution of the book lies in its integration of media studies with the field of policymaking. As pointed out by Spitzer in his introductory essay, the two fields have developed independently with little cross-fertilization of ideas. Contributor Julio Borquez notes that leading models of policymaking (e.g., the garbage-can approach of John Kingdon) curiously downplay the role of the media. Alternative perspectives such as iron triangles and issue networks include no explicit recognition of political communications. The result is that policymaking studies devote much greater attention to Congress, the presidency, policy entrepreneurs, political parties, and interest groups than to the news media even though the latter construct the interpretations that are so vital to the behavior of other actors.

In this book, the analysis of the media–policy nexus is organized in three parts. The first part provides a theoretical perspective by Doris Graber on the extent to which the media support or subvert the political status quo. Her conclusion is that there is evidence of short-term effect on individuals and groups but that absent sustained and saturation coverage, many factors limit the ability of the media to alter the status quo at the systemic level.

The second part of the book presents case studies of media impact on domestic policymaking. Particularly useful are analyses of Federal Communications Commission decisions from 1975 to 1990 by Wenmouth Williams, Jr.; media agenda-setting in regard to economic development by Michael Hawthorne; invasion of the right to privacy in a media-driven era by Dean Alger; media images of environmental policy by Jerry and Michael Medler; and the use of racial appeals in the 1990 Harvey Gantt–Jesse Helms Senate race in North Carolina by Montague Kern.

These case studies make the point that the media cannot determine outcomes but that by the manner in which particular interpretations are put forward and symbolic imagery expressed, subtle impressions can be fundamentally altered. In these ways, reporters exercise substantial sway over how policies are formulated.

The third part reviews studies of the media in a comparative perspective. In this section are pieces on source selection by Robert Sahr, AIDS reporting in the United States and Britain by Holli Semetko and Edie Goldenberg, South Africa reporting by Marion Just and Ann Crigler, and media influence on U.S. foreign policy by Patrick O'Heffernan.

The Sahr piece is a powerful demonstration of media power through the mechanism of "credentialing experts" (p. 153). By the types of individuals chosen as commentators and experts, reporters determine how stories get framed. As Page and Shapiro (1991) have demonstrated, these choices have consequences. Citizens view commentators and experts as more independent than many other sources and this credibility heightens the ability of experts to mold public reactions.

Researchers will also find fascinating the comparative coverage of AIDS in the United States and Britain by Semetko and Goldenberg. Using surveys of reporters in the two countries, the authors find dramatic differences in reporting on AIDS. Because American reporters function in a highly decentralized market, there was greater variation in press response to AIDS than in the much more highly centralized British media system.

This book has much to recommend it. Scholars will appreciate having, in one volume, a range of media case studies on timely subjects. Individual selections are well written and clearly organized. The authors have compiled informative case materials. For those seeking a review of domestic and comparative policymaking, this volume provides a plethora of material.

My only complaint about this book is its relative inattention to the structural revolution currently taking place in the media industry. The rise of cable, the use of satellite technologies, the 30-percentage-point loss of audience share by the three major television networks, and the emergence of new networks is an enormous change from a system dominated by three networks and a few prestige newspapers.

Competitive pressures based on these structural changes are driving the media toward more tabloid coverage, and this has blurred the boundaries between news and entertainment. As a result, citizen confidence in the news media has been undermined. When the *National Enquirer* has the means to dictate coverage by the *New York Times* (as has occurred repeatedly in recent years), it is a dramatic sign that the core no longer dominates the periphery of the media system. We owe it to ourselves as scholars to determine how these important structural transformations will affect public policymaking.

Brown University DARRELL M. WEST

Black Faces, Black Interests: The Representation of African Americans in Congress. By Carol M. Swain. Cambridge: Harvard University Press, 1993. 275p. $37.50.

From Protest to Politics: The New Black Voters in American Elections. By Katherine Tate. Cambridge: Harvard University Press, 1993. 221p. $32.50 cloth, $15.95 paper.

Carol Swain and Katherine Tate introduce a great deal of important new data to the African-American/black politics literature and propose distinctive approaches to our understanding of black political values and political behavior. Swain considers substantive and descriptive representation in congressional politics. Tate examines black political attitudes and values as the group has shifted from forcing its way into the political system through collective protest and confrontation to coming to terms with the complexities of party, electoral, and institutional politics. Although they address comparable issues and use different methodologies, Tate and Swain reach considerably different types of conclusions primarily because of their distinctive philosophical approaches to the study of black politics.

Swain examines a political subject of great complexity—representation of racial interests—in her study of the processes by which black members of Congress are elected and by which they represent their constituents in congressional districts of varying racial proportions. She also reviews the processes by which black districts elect black or white representatives. She critiques the appearance of group-based orientations in vote-mobilization strategies, in drawing congressional districts, and in public policy formulation.

Tate reviews the philosophical and theoretical perspectives held by this constituency about electoral politics and public policy, which measure African-Americans' attraction toward, or rejection of, public life and value. Tate inverts the goals of her study from the more conventional examinations of the impact of blacks on American political life, concentrating instead on how their efforts in recent decades to integrate national politics and political institutions have shaped and reshaped their own political values. Tate analyzes data collected by the Program for Research on Black Americans (PRBA) at the Institute for Social Research at the University of Michigan. The PRBA conducted 1984 and 1988 National Black Election Studies in which "1150 voting age Blacks were interviewed by telephone prior to the 1984 national election, and 872 were reinterviewed prior to the 1988 national election" (p. 19). Another 473 were reinterviewed before—and 418 after—the 1988 election. Gurin, Hatchett, and Jackson's *Hope and Independence* (1989) is based on the 1984 survey.

Swain studies descriptive and substantive representation by assembling a series of profiles of black and white members who represent districts of varying racial combinations: majority-black districts (including historically and newly black, heterogeneous districts) and majority-white districts. She codes districts according to *contemporary* racial characteristics rather than the characteristics at the time the district first elected a black representative—which removes some districts from the majority-black category. This weights her findings in favor of her argument that black representatives do not face difficulty winning election outside of majority minority districts, more than is merited by the data.

Swain combined qualitative examination of the representative and his or her performance in office, both in Washington and in the district, with quantitative measures of their voting on redistributive and civil rights issues for the 100th Congress (1987–89). In the development of her qualitative materials, Swain the researcher is the most important instrument in evaluating the members as she assembles biographical and political profiles, describes their districts, evaluates their political strategies, reports on their performance in the Congress, judges their overall effectiveness in representing their constituencies, and comments on civil rights activists' choice to design black districts. Swain's profiles vary quite dramatically in tone. The variance in sentiment seems to arise from several sources: whether the members granted her an interview and the opportunity to observe them at work, whether they represent majority-black (especially historically black) districts, and whether they aggressively seek white electoral support. Swain is also quite sensitive to class issues: she is critical of representatives she perceives as middle- or upper-class, although her affect toward them seems somewhat mediated if they make strong appeals to white voters. She concludes that black representatives from majority-black districts do not work as hard at representing their constituents as do whites or blacks from majority-white districts but offers no hard measures to substantiate her conclusion.

It is unlikely that any researcher could avoid some personal reactions to the subjects under study, but Swain clearly has a preference for certain types of racial political strategies. Any representative who chooses these strategies (e.g., direct appeals to white voters, especially in a non-majority-black district) is praised. Any representative who appeals strongly to black voters in a majority-black district is critiqued.

She is also highly critical of majority-black districts, a strategy adopted by the voting-rights bar to increase black opportunities for descriptive representation. Swain even argues that majority-black districts have an independent impact on crime rates: "Large concentrations of poor black voters . . . are often plagued by high crime rates" (p. 203). Such districts have recently become a subject of increasing controversy and deserve careful review if they are proposed as the sole strategy for improving black political representation. Swain concludes that the election of black representatives from nonblack districts is proof that majority-black districts are not a prerequisite for blacks' election and that whites do not vote in racially polarized fashion to defeat black candidates. In fact, her case studies illuminate the considerably greater complexity of electoral contests when race is involved, a factor that affects both the success ratio of black candidates and the seriousness with which candidates debate policies of interest to black voters.

There has been much speculation that the development of greater class differentiation among blacks would lead to growth in political conservatism and in support for the Republican party. Neither Tate nor Swain finds evidence for this. Tate also shows that socioeconomic diversity has changed and made more complex—but not eliminated—the importance of race, civil rights, and group identification (group fate) for African-Americans: "Fully 75% of Blacks in the survey felt that what happens to Black people will shape their lives" (p. 25). Upward mobility has a "mixed effect on one's racial identity, simultaneously undermining it and reinforcing it." The college-educated therefore have the strongest common-fate identity: those with manifest middle- or upper-class identification scored less strongly, while income was statistically insignificant. On policy measures, socioeconomic status "strongly affected Blacks' policy attitudes" (p. 46), with the higher status less likely to support higher spending on social welfare, food stamps, public schools, and medicare. Blacks are also much more likely to be conservative on public morality issues. Yet Tate concludes that only 10% "may be" economic conservatives (p. 48).

The norms of empirical research, at base, require the researcher not to allow personal values to confound the findings. Swain's work has this fault, much of which has to do with the way she decontextualizes her research. She also makes highly critical, even pejorative, statements and other observations, absent adequate documentation or substantiating background information (pp. 90–91, 171). Researchers of African-American politics must understand the historical context within which African-Americans function politically in order to have a grasp of their distinctive values and uses of politics. That context is much broader than conventional American political science, as Lucius Barker noted in his 1993 presidential address to the American Political Science Association.

Swain contextualizes her analysis of the performance of African-American and other representatives in their districts, solely within the political science literature on Congress and on voting behavior. Tate's study clarifies how one can integrate these dimensions. It is thoroughly grounded in the political science literature, and it explores the historical traditions that have distinctively shaped black political attitudes and behavior. In chapters on group resources and on black power, for example, Tate describes how African-Americans managed and organized their "politics" in a society that prohibited democracy across racial lines.

Tate's analysis of blacks and the Democratic party provides an invaluable introduction to the importance of race in the national party's politics after the 1930s. She also identifies the independent role that black political activists took in forcing the party to reorganize its policy agenda and political appeals from the 1960s through the 1980s, thereby challenging Carmines and Stimson's treatment of that subject. In her analysis of the 1984 and 1988 data, she found few significant differences in the policy attitudes of black Republicans and Democrats; and blacks were about equally likely to be Democrats, regardless of income. Gender and age are significant differentiating factors affecting partisan identification, with men and younger cohorts more likely to be political independents or Republicans. Tate shows that the Jackson campaign had a conflicting impact on blacks' relationship to the party, with increases in blacks' efficacy and position within the Democratic party and in their dissatisfaction and distrust after Jackson's loss at the conventions. However, because of the sharp rightward movement of the Republican party during the Reagan presidencies, Tate found no appreciable change in their interest in the Republican party.

By contrast, Swain's critical treatment of a series of members of the Congressional Black Caucus and of the caucus as a group addresses the historical configuration of black political life subjectively. Black group identification is normative and problematic, rather than an empirical issue. Swain treats any appearance of group-based mobilization and black nationalism as aberrant—even immoral—behavior, while white racial attitudes are not a major factor. She misunderstands the significance of group identification for substantive policy in previous literature on the caucus: "Marguerite Barnett's indictment is most severe. . . . The caucus is failing in its mission to deliver the types of legislation most needed by blacks" (p. 38). Barnett, in fact, argued very differently from Swain's central thesis: "The Congressional Black Caucus, in order to be effective within the electoral context, would have to understand and directly attack the structural conditions of black subordination. Otherwise, structural constraints will continually undermine seeming advances won through electoral politics and incremental strategies" (Barnett 1982, 52).

If there are weaknesses in Tate's study, they fall within her chapter on black power and electoral politics, which is somewhat cursory in its treatment of black power. That chapter nevertheless contextualizes the experiences and efforts of black elected officials who were learning as a group, for the first time in the 1970s and 1980s, how to represent. The creation of an electoral cohort where there had been none before generated a complex set of new political issues for blacks, individually and collectively.

Tate's finding that support for black nationalism increases as conditions within the African-American population deteriorate helps explain the variables that shape its strength over time and within the population at specific points in time. Nationalism therefore has greater strength among poor blacks and the less-educated and has increased as socioeconomic conditions have worsened in recent decades. Those who strongly express nationalist views are also less likely to participate in electoral politics.

Tate and Swain conclude their studies with discussions of similar subjects (the problems of leadership within the black population, nationalist issues, and conflicts over majority black districts); but the overall framework within which they place these issues and the objectivity with which each approaches her subject are profoundly distinct. Swain reports on what she thinks ought to happen—that blacks need not limit their political campaigns to predominantly black locations and that whites can represent African-Americans even more effectively than blacks. She reaches conclusions not fully merited by her data and simplifies a very complicated political phenomenon. Tate balances empirical social science with historical perspective. She clarifies that while the black population has moved from protest to politics, racial self-identification is still strongly valued within the group; and dissatisfaction with social, political and economic status crosses class and party lines. The African-American population is in no sense undifferentiated: variations in levels of education, economic status, gender, and age reveal important partisan preferences, attitudes towards electoral politics, and support for Jackson. Protest and racial politics will therefore continue in national life. African-Americans and all other Americans have just begun to learn the difficulties involved in balancing descriptive and substantive representation.

University of Illinois, Urbana DIANNE M. PINDERHUGHES

The Making of American Exceptionalism: The Knights of Labor and Class Formation in the Nineteenth Century. By Kim Voss. Ithaca: Cornell University Press, 1993. 290p. $39.95 cloth, $16.95 paper.

In her new book, Kim Voss, an historical sociologist, addresses a hoary issue in U.S. historiography, the theme of "American exceptionalism." Simply put, the concept of "American exceptionalism" posits that the United States, unlike all other industrial societies, has lacked a working class conscious of itself and hence capable of advancing its interests through a mass socialist or labor political party. Voss instead argues that until the end of the 1880s workers in the United States, England, and France followed a common path "as activists in each country built new inclusive unions and endorsed new radical creeds in reaction to similar fundamental transformations in the organization of work and social life" (p. 234).

Voss's book consists of two parts, the first an overview of the history of workers and labor movements in nineteenth-century England, France, and the United States; the second, a case study of the Knights of Labor in New Jersey constructed from a statistical analysis of 323 local assemblies. Voss uses the research of labor and social historians to demonstrate that from the initital emergence of market-driven capitalist societies in England, France, and the United States until the end of 1880s, workers in all three nations acted similarly. During what she characterizes as the first moment of class formation, the era between the 1820s and 1840s, skilled workers, who were exploited by their masters, built craft unions and political protest movements. The subsequent growth of industrial capitalism diluted artisanal culture and its craft unions. In what Voss characterizes as a second moment of class formation between the 1840s and 1880s, semi-skilled factory operatives came to dominate the labor force and the further development of trade unionism depended on the ability of skilled workers to ally with the less skilled through new forms of organization. According to Voss, the "great depression" of the late nineteenth century resulted in "a significant convergence of experiences and grievances across all sectors of the working class" (p. 70). She suggests that in the 1880s English workers created a "new unionism" that drew less skilled workers into a labor movement inclined toward socialism; that French artisans organized their nonskilled compatriots into militant community-based labor federations that adopted revolutionary syndicalism; and that American workers built the Knights of Labor to amalgamate the skilled and less skilled for common economic and political action on the principle of "working-class republicanism." Thereafter, labor's trajectory diverged in the three nations. In England and France the "new unionism" and radical working-class politics survived to promote the British Labour Party and French syndicalism; in the United States, however, the Knights of Labor collapsed, and was replaced by the anachronistic craft unionism and antipolitics of the American Federation of Labor. The failure of the Knights, Voss suggests, "marks the moment when, from a comparative perspective, the American labor movement began to look exceptional" (p. 235).

Voss uses the history of the Knights in New Jersey to prove her hypothesis concerning the origins and timing of "American exceptionalism." Working with local newspapers, state and federal reports, census data, and a variety of local labor history archives, she constructs a composite portrait of industries, cities, and the Knights of Labor in New Jersey. Her data-gathering and sophisticated statistical techniques proved to my satisfaction that the New Jersey Knights was an organization that recruited few non-working-class members; that its skilled workers acted as spark-plug unionists to recruit their less skilled brothers and sisters; that the Knights organized primarily on the basis of industry and craft rather than as mixed assemblies; and that the organization was strongest in larger, more diverse, and economically advanced cities where interactions among heterogeneous workers fostered a community-based consciousness. Voss's portrait of the Knights in New Jersey renders them comparable to the British "new unionists" and the French Syndicalists in their desire to liberate workers from wage slavery. What distinguished U.S. workers and their labor movement from their European analogues was not the singularity of American historical development (exceptionalism) but rather the inordinate power and antiunionism of U.S. employers. More so than any previous scholar of the Knights, Voss demonstrates that neither organizational paralysis, weak leadership, the antagonism of craft unionists, the ethnoreligious diversity of workers, nor the alleged utopian middle-class character of the Knights caused its failure. Examining a struggle between employers and Knights in Newark in 1886–87, Voss shows how the rising strength of labor prompted employers to form an association that precipitated a conflict with the Knights during which public authorities helped capital crush labor. What happened in Newark, asserts Voss, was writ large across the nation wherever and whenever employers organized to fight the Knights. Having lost the class struggle against employers during the second moment of class formation in the United States, the nonskilled workers found themselves bereft of organization while their skilled brothers retreated into defensive enclaves of conservative, antipolitical craft unions (the AFL), thus establishing the conditions for "American exceptionalism."

Voss's portrait of the New Jersey Knights, their history, and the causes of their failure rings true. Her analysis of "American exceptionalism" raises questions. Like many other contemporary historians, I find "exceptionalism" a tired concept, one that explains both too much and too little about U.S. history. Like Aristide Zolberg, I believe that every national history is in part "exceptional" and that there is no single historical model against which to measure a nation's development and declare it wanting. I think that Voss also exaggerates the differences between French and British labor, on the one hand, and American labor, on the other, after the collapse of the Knights. The "new unionism" in Britain remained relatively weak until World War I while the British Labour Party and socialism scarcely attracted most workers. In France, unionism remained feeble and most semi-skilled workers lacked unions until the 1930s when France experienced its own labor upheaval during Leon Blum's popular front version of a New Deal. My reading of French history suggests, moreover, that employers and the state were as antilabor as their U.S. counterparts. Indeed, between the 1880s and 1920, the U.S. had a more powerful union movement than France, and a socialist movement at least as large and influential as those in France and England. A final aspect of Voss's comparative approach troubles me. I understand her choice of England for purposes of comparison. But why France rather than Germany, when the latter nation, according to most economic and business historians, had an industrial history and structure most like that of the United States and when it is conventionally considered the model for socialist and trade union development from which other more "exceptional" nations allegedly deviated?

Binghamton University MELVYN DUBOFSKY

Regulation in the White House: The Johnson Presidency. By David M. Welborn. Austin: University of Texas Press, 1993. 354p. $45.00 cloth.

David Welborn's discerning examination of the ways in which President Johnson conducted his "business in regulation" sheds much needed empirical light upon the translation of presidential power into regulatory behavior, from the public image projected by a board or bickering commissioners, to the substantive details of international airline routes and residual oil controls. Welborn's analysis provides a crisp picture of one president's perceptions of his regulatory responsibilities and the context within which he operated, defined (somewhat oddly) by time constraints, political necessity, an ambitious political agenda, and a hefty respect for the autonomy and competency of the independent regulatory commissions. Yet Welborn's concentrated effort to study regulation in the White House yields insights into the institutional powers of the presidency that extend beyond the business of regulation, and that illustrate a president's understanding of the public interest, his mandate to serve, and the implications for exercising presidential authority.

Welborn's study builds from a simple premise: The president has inescapable regulatory responsibilities related to his role as chief executive, as a leader with a legislative agenda, and as the designated authority for deciding particular substantive regulatory matters (pp. 4–6). Welborn asks, in exercising these various responsibilities, "what do presidents actually do and how do they go about doing it?" (p. 256). He argues that the business of regulation in the Johnson White House was motivated by three objectives: 1) to prevent harm that regulatory activities might bring to the administration; 2) to "enlist regulatory assistance" to facilitate the administration's "basic economic policy objectives," and 3) to pursue "consumer, health, safety, and environmental protection" by expanding social regulation (p. 21). These objectives were pursued in a manner that "confounds the conventional wisdom about presidents and regulation and also about Johnson the president (p. 19). Welborn documents a White House actively involved in managing its regulatory business at a variety of levels, influential in its ability to affect regulatory outcomes, and a president whose depth of substantive and political knowledge of regulatory affairs provided ample means to facilitate a broader political agenda. Regulatory business, in other words, was not an end in itself for Johnson, but a means to broader ends.

Following an introduction providing an overview of the institutional and political constraints affecting presidential management of the regulatory process, and a profile of LBJ's leadership style, the book probes Johnson's regulatory role in three contexts. Chapters 2 and 3 focus upon the independent commissions. An examination of Johnson's strategic use of the appointment process, and the "linkages" between the White House and the commissions on a day-to-day basis provides a convincing argument for the "balance" Johnson tried to achieve "between agency and presidential business," and between an activist White House seeking cooperation and teamwork with the commissions, and the need to respect commission autonomy (p. 25). Regulatory activity within the executive branch is examined in chapters 4 and 5. In contrast to Johnson's "arms length" rule in dealing with the substantive agendas of the commissions, the White House played an integral role in the regulation of oil imports through the Department of Interior, commodity prices through Agriculture, and the coordination of banking supervision through Treasury. Further, these regulatory leaders readily assumed the roles of "advocates," in contrast to the expectations for commissioners. The direct links between these areas of regulation and economic performance (particularly the effort to hold down inflation), brought the White House into the substantive details. In matters of antitrust (chapter 5), however, the ties between the White House and the Justice Department, as well as the perceived connection between antitrust policy and economic performance, were more "tenuous." Chapter 6 examines the administration's legislative agenda, particularly the ambitious expansion of social regulation. The final chapter categorizes Johnson's regulatory involvement across the three contexts as "high," "moderate," or "low," and examines the presence of three factors in individual cases that tended to draw Johnson into regulatory issues: "duty, compulsion, and disagreement" (p. 261). When the president had a de jure or de facto "obligation to act," a compulsion to act for furthering his (political) interests, and there was disagreement between the White House staff and an agency, for example, Welborn argues there was a likelihood of high presidential involvement in the issue.

Welborn's use of the memoranda and working papers of the Johnson White House provides direct links between presidential priorities, the use of presidential influence, and (often) the shift in regulatory behavior—at times substantive, at times stylistic. Yet it is precisely his very detailed empirical work that would lend itself to a much richer and more generalized analysis of the institution of the presidency as an influence upon regulatory activities, in particular, but the administration of the executive branch, more generally. This reviewer was hoping for a more systematic return to questions raised early in the book about presidential influence over regulatory behavior, versus that of regulated interests and the Congress. What does the Johnson experience tell us about the limits of presidential influence, and are those limits personal (related to style and agenda) or institutional? If the authority is there to be exercised, what determines its restrained use? The complicated debate over "who controls the bureaucracy" (long fought on the field of formal models) could be enriched by an analysis such as Welborn's that demonstrates the exercise of influence as well as its limitations. Further, how does Johnson's approach to the business of regulation contrast or parallel the approaches of more recent presidents, particularly Reagan? Was Johnson's use of the BOB, and his formula for regulatory appointments a precursor to the more centralized and systematic approach to regulation taken by later presidents? A second, minor criticism is Welborn's hesitancy to elaborate upon Johnson's governing style, such as his willingness to buck the demands of "special" interests in the attempt to facilitate fairness. We often argue that presidents are motivated by their more broadly based political coalitions in their conduct of executive branch business (in contrast to the parochial concerns of Congress), but a systematic investigation of their actions in trying to serve that broader interest (particularly when confronted by powerful, more particular interests) is scarce. Welborn provides some insights, although without elaboration.

This discussion about the potential for extending Welborn's analysis should not take away from his important contribution to the study of regulation and his vivid presentation of the very personal and political management of regulatory affairs that is possible from the White House. The book is thorough in its coverage, consequential in its findings, enlivened by its empirical base (in part, the quotes and commands of LBJ on regulation), and a must-read read for anyone interested not only in the business of regulation, but in the institution of the presidency as a tool for managing, directing, and influencing bureaucratic behavior.

University of Wisconsin, Madison Anne M. Khademian

From Outrage to Action: The Politics of Grass-Roots Dissent. By Laura R. Woliver. Urbana: University of Illinois Press, 1993. 195p. $36.95.

Laura Woliver analyzes four Wisconsin community controversies that transpired between 1977 and 1984 when citizens reacted angrily to perceived failings in the criminal justice system. Two of the controversies stemmed from egregious remarks made by local judges about the victims of sexual assault. Dane County Judge Archie Simonson blamed sexual assault on the provocative way that women dressed. He was swiftly ousted in a recall election. On the other hand, Grant County Judge William Reinecke narrowly survived a recall election after suggesting that a five-year old girl had made sexual advances toward a man who molested her. Woliver's third case revolved around the death of an African-American man, Ernest Lacy, who was beaten by Milwaukee police officers and died while in their custody after being arrested on suspicion of rape, a charge that later proved unfounded. Following sustained community pressure, five Milwaukee police officers were either fired or suspended for their part in the Lacy death, and the Lacy family eventually won financial compensation through settlement of civil suits they filed. The fourth case of collective action that Woliver studies was motivated by the abduction and murder of a ten year old girl named Paula McCormick by a man who had a history of criminal conduct, including an earlier conviction for sexual assault. Community pressure placed discussion and study of the criminal justice system on the agenda and prompted meetings between public officials and citizens on this subject, but did not produce many concrete changes.

Woliver's book adds usefully to our catalogue of studies of community activism. In tracing each case, she supplements newspaper accounts with material gathered from in-depth interviews conducted in 1984–85 with leading participants. She chronicles the development of the movements; discusses how the incidents were portrayed in the media; analyzes the process of issue definition, agenda setting, and coalition formation; reports the views and reactions of participants; compares the different outcomes of the movements; and reflects on the impact of political activism and the ability of ordinary citizens to be heard. Throughout, Woliver makes an admirable effort to apply to her cases insights drawn from a wide range of literature on social movements and political activism.

There is, unfortunately, not always a clear summary of the events surrounding each of the cases. Sometimes the facts of the precipitating event trickle out in the course of her analysis, instead of being presented up front. The most serious instance of this occurs in the chapter on the Simonson recall, where crucial details of the Judge's remarks during the sentencing hearing that instigated the public furor are not reported in the introductory sections, but rather more than half way into the chapter.

In the Simonson and Lacy cases, the activists were largely successful in reaching their objectives. Woliver argues that community activists in those instances were able to draw respectively upon the broader feminist and civil rights communities in support of their goals. The Simonson recall effort drew heavily from experienced women's organizations (including the National Organization for Women) that were centered in the liberal Madison university community. The Lacy protests combined dozens of existing political, social, and religious organizations that banded together in a broad coalition. Both movements successfully tied the local incidents to the larger feminist and civil rights agendas.

In contrast, the organizers in the less successful McCormick and Reinecke cases appear to have been less imaginative political entrepreneurs. They were unable to frame the controversies using the kind of general ideological language that would have activated wider support for their cause. Woliver contends that their inability to ground the movements in an established activist community not only undermined political mobilization, but made it harder for those who did participate to receive the social support they needed to retain their convictions. In addition to contrasting degrees of organization, there were several mundane political reasons why the Simonson recall succeeded whereas the Reinecke campaign didn't. The Reinecke recall election was closely contested, but Reinecke survived because he ran in a more conservative county and there was no strong alternative candidate to him as there was in the Simonson recall. Simonson also had a more checkered past record that added to his vulnerability, in contrast to Reinecke who was more highly respected by the local elite, including the legal profession. The evidence also indicates that Reinecke's misconduct was judged by opinion leaders and the public to be less serious and offensive than Simonson's.

A few activists involved in the Reinecke and McCormick controversies told Woliver of their bitterness over their experience with local politics. The McCormick activists felt that politicians had given them the runaround. Those who worked to remove Reinecke recalled facing hostility and accusations of political extremism, which stung all the more in the small town atmosphere of Grant County. Perhaps the inexperienced anti-Reinecke forces were naive to think that they could wage their campaign without generating counterattacks from the opposition, since the attacks on them were not unlike some of the charges hurled at the leaders of the Simonson recall. One of the main conclusions Woliver draws from her analysis of the four movements is that some interests will remain unorganized because individuals fear social censure in communities where they feel outnumbered. While this must often be true, neither the Reinecke or McCormick case appears to bear heavily on this point. Although the Reinecke campaign was hard fought, with harsh rhetoric used by both sides, it does not seem from Woliver's account that the local majority "silenced" (p. 138) the dissenters as she claims in her conclusion. It is difficult to see how the activists could have felt unduly isolated and stigmatized given the relative ease with which they gathered thousands of signatures for the recall petition and the well-publicized calls for Reinecke's removal issued in major state and national newspaper editorials. As it happened, the activists came very close to winning their campaign and, even in defeat, appear to have modified the Judge's subsequent behavior on the bench in a manner to their liking.

Similarly, I was not convinced by Woliver's claim that the activists in the McCormick movement had been "hushed" (p. 111) by insincere politicians. Organized protest was stalled because there was no obvious culprit in the criminal justice system who could be fingered and held responsible for the girl's murder. Local politicians may have, as Woliver claims, stalled the activists with meetings, symbolic gestures, and speeches, but the activists were also sobered by the difficulty of trying to reform a criminal justice system that they only vaguely understood. Not all was lost by any means. Participation in the local movement allowed people to show solidarity with their neighbors in expressing outrage over what had happened in their community. Woliver speculates nevertheless that the negative experiences of the few activists she interviewed might discourage any future endeavors, but this seems an overly pessimistic conclusion if applied to the entire movement. One wonders what would have happened if there had been a second McCormick case on the heels of the first. Quite possibly the coordination of protest would have been more rather than less likely given the networks created and lessons learned in the original case. A factor that primed participation in the campaign to recall Simonson, after all, was the recent occurrence of several rapes in Madison, which had made sexual assault a topic of discussion in the media and the public and thereby heightened sensitivity to Simonson's comments.

In general, I was surprised that the interviews did not seem to play as central a role in the analysis as I imagined they would. They provide insight into the feelings and perceptions of activists, but they are not used to make any particularly telling theoretical points. Although it is reported that Judge Simonson was interviewed, I could not find reference to that conversation in the chapter on the case. Also, the small number of interviews conducted were concentrated on protest leaders, leaving us to speculate about the motives and reactions of the more casual participant. One issue raised in the Simonson controversy, for example, was that the activism was motivated partly by racism, given that the victim was white while the accused juveniles were black. The only evidence offered against this proposition is the denials of recall leaders. Similarly, the handful of leaders interviewed from the Reinecke campaign were disillusioned by the rough treatment they received, but was their reaction shared by the average participant who was not on the front lines of the dispute and who, therefore, may have enjoyed a more favorable cost-benefit calculus? In contrast to the results from her interviews, newspaper reports following the Reinecke recall election cited by Woliver (p. 137) indicate that some participants were content with having given the Judge a scare. On these matters, the in-depth approach, while providing valuable detail and a unique perspective, may actually have been too close to the action, sacrificing representativeness and obscuring more general observations and conclusions.

Northwestern University DENNIS CHONG

COMPARATIVE POLITICS

God's People: Covenant and Land in South Africa, Israel, and Ulster. By Donald Harman Akenson. Ithaca: Cornell University Press, 1992. 404p. $29.95.

Unsettled States, Disputed Lands: Britain and Ireland, France and Algeria, Israel and the West Bank–Gaza. By Ian S. Lustick. Ithaca: Cornell University Press, 1993. 576p. $37.50.

How are we to explore cases of intransigent conflict in societies where ethnicity, race, and religion are, at the core, group differences? Theoretically rooted comparison is increasingly relied upon by scholars, political activists, and conflict resolution practitioners in thinking about what were once understood as a series of unique situations. The two books under review here are interesting in that despite the fact that Ireland and Israel are two of the three cases that each treats, the specific theories driving the comparisons are very different. As a result, one sees how different theories, when applied systematically, lead to very different—but not necessarily incompatible—insights.

Akenson, a historian, focuses on the role of what he calls "covenantal thinking" as it has shaped the political beliefs and actions of three dominant groups: Afrikaners in South Africa, Jews in Israel, and Protestants in Northern Ireland. The covenant that lies at the core of the world-views of each of these groups is told in the Exodus story in the Old Testament. Its "if–then" character, "If Israel will be his people, Yahweh will be their God" entails obligations and responsibilities for all Chosen People that are manifest through an emphasis on social law, a clear distinction between enemies and allies, the warlike character of the Deity, attachment to specific pieces of land, and group purity (p. 13).

Lustick's main concern is in the dynamics of state expansion and contraction associated with shifts in boundaries. He explores how Britain and France went from seeing Ireland and Algeria as integral parts of their national territory to viewing Catholic Ireland and Algeria as independent political entities. His analysis of these two cases then provides the basis for an examination of the prospects for the creation of an independent Palestinian state on the West Bank and Gaza.

Despite the different core questions driving these studies, both authors address a number of common issues. For example, each is concerned with the central role of cultural metaphors and shared interpretations of the social and political world. Akenson develops the idea of the *covenantal grid,* a conceptual schema that offers a culturally constructed explanation for past events and a guide to future action for the politically dominant groups in each society under study. He explores the grid's biblical roots and explains its social and political power in these three societies through both the general fit between the grid's central metaphors and the human condition and its more specific capacity to provide relevant guidelines for social and political action and group survival.

Land, exodus, and blood sacrifice are three central "points of great sensitivity within the grid and anything that touches one of them energizes the entire grid and calls forth an immediate, focused, sometimes violent response" in each culture under examination (p. 31). In his discussion of political myth, Akenson, like Geertz, suggests that myths both reflect everyday societal functioning by providing a psychocultural framework for the interpretation of the world and, at the same time, shape the social and political order.

Lustick, interested in shared, culturally constructed beliefs about the state and the political order, inquires into the sometimes convulsive dynamics associated with state boundary redefinition. In doing so, he reminds us that while we readily think of boundaries as unchanging, the historical truth is that state boundaries are political constructions. The expansion and contraction of states, he shows, occurs more often than we believe, accompanied by political upheaval in which the foundation of the political order and make-up of the political community are at stake. His analysis, built upon Gramsci, emphasizes three stages (incumbency, regime, and ideological hegemony) and two thresholds involved in state expansion or contraction. The *ideological threshold* involves struggles over definition of the political community, while the *regime threshold* marks struggles over the structure of the political order.

Akenson and Lustick emphasize very different—but not necessarily incompatible—mechanisms for understanding the politics of domination and change in the cases they examine. Akenson pays more attention to the culture of the dominant group, and Lustick stresses the political dynamics in the dominant state. Akenson makes a strong case for the power of culturally shared beliefs as motivators of action through the construction of in-group solidarity and the development of in-group institutions and practices to exercise power and ward off outside enemies. He points out that none of the groups he examines has shown any real interest in gaining new adherents through conversion, despite the fact that all consider themselves as besieged minorities. In fact, there is great concern, almost obsession, with the problems of impurity and dilution of the group through too easy and too-casual contact with outsiders.

A crucial mechanism Akenson identifies for achieving social and political separation is through religious institutions and practices that inform daily life. Also critical is the role of language: he reminds readers that both Afrikaans and Hebrew were dead languages, a few generations ago only spoken by a handful of people. Language, like other exclusive cultural practices, comes to serve as an emotional focus for group solidarity but also becomes the central mechanism for achieving that solidarity through its central role in education and socialization of successive generations. (Interestingly, in Northern Ireland, where language does not separate Protestants and Catholics, there are separate school systems for each group offering distinctive content and values to students.)

Lustick's inquiry into the process of changing state boundaries begins with detailed discussion of an important problem—the extent to which Israeli control over, and settlement in, the West Bank and Gaza has created "facts on the ground" that effectively preclude the emergence of an independent Palestinian state in the area. His conclusion is that while facts on the ground may be difficult to undo, they are not enough to fix political boundaries in the absence of both ideological hegemony and an effective regime. Crossing both of these thresholds, it turns out, was crucial, in somewhat different ways, in ending British rule over all of Ireland and French control of Algeria.

He details both the movement across the ideological threshold in Britain in the 1830–86 period, when the idea of Irish self-rule moved from the absurd to government policy, and the changing conception of where and what was France from 1830 to 1958. In each case, the collapse of a particular hegemonic definition of the state as necessarily including the relevant peripheral regions provoked a second conflict, namely, over the structure of the regime itself, which, for a time, takes precedence over issues of incumbency and power holding.

State contraction (what Lustick calls *rescaling*) occurs in different ways in response to specific challenges. In Britain, problem decomposition (dividing Ireland into two parts) and in France, regime recomposition (the birth of the Gaullist Fifth Republic) turn out to be crucial mechanisms to the management, if not the resolution, of the regime and ideological crises. These cases, he persuasively argues, have important lessons for the Israeli situation; and he proceeds to develop some thoughtful and imaginative scenarios about the future of the Middle East (some of which have already come to pass) based on his theoretical analysis.

Despite the popular image that caricatures covenantal cultures and colonial societies as head-in-the-sand, ostrichlike creatures always resistant to change, both studies show this view as too simplistic. Akenson devotes three chapters to the significant shifts in the political landscape in Northern Ireland, South Africa, and Israel in recent years. He argues that while covenantal cultures' immediate response to outside threat is to assume they can outlast it, new relationships can be built, and great changes are possible within the covenantal grid when these cultures are persuaded that a friend (or foe) is there to stay. However, he warns that "because within covenantal cultures compromise is taken as a sign of weakness, the leaders of these nations must be given plenty of room to talk to themselves and their constituents, to become accustomed to the potential for change, and to convince themselves that the idea was theirs all along" (p. 356). Lustick, too, speaks to the issue of change, arguing that sometimes it is sudden, swift, and nonlinear, as one can see in the transition between the Fourth and Fifth French Republics.

Both books under review are important studies showing the merits of theoretically driven, historically rooted, comparative analyses. Akenson's will probably be viewed as more unconventional by political scientists skeptical of the power of ideas (especially religiously rooted ones) to shape political order in the modern world. Although Lustick, too, emphasizes political actors' conceptualization of their worlds and how this shapes the alternatives they take seriously, his analysis will be more familiar in terms of both its concepts and its problems. Both books are long—in Lustick's case, I think, longer than necessary. Each provides theoretically informed cases and shows the value of comparing what often, to both participants and observers, seem like unique, unrelated cases.

Bryn Mawr College MARC HOWARD ROSS

The Domestic Politics of German Unification. Edited by Christopher Anderson, Karl Kaltenthaler, and Wolfgang Luthardt. Boulder: Lynne Rienner, 1993. 254p. $28.00.

From Bundesrepublik to Deutschland: German Politics after Unification. Edited by Michael G. Huelshoff, Andrei S. Markovits, and Simon Reich. Ann Arbor: University of Michigan Press, 1993. 396p. $55.00 cloth, $22.50 paper.

The unification of Germany is clearly one of the most significant events of the twentieth century. These two new edited volumes take stock of the changes in Germany's internal and external politics and their meaning and begin to piece together a portrait of the "new" Germany. In spite of striking differences in their approaches, both collections make it clear that the united Germany is more than the sum of its parts, though the complexity of unification and its status as a "work in progress" make it difficult to define precisely the character of the new Germany.

From Bundesrepublik to Deutschland is the more ambitious of the two collections. The volume considers the contributions that different research agendas in comparative politics and international relations have made to our understanding of German politics, and then questions their applicability to the study of Germany after unification. Huelshoff and Markovits note that prior to 1989, the study of German politics evolved into the study of a "normal" democratic regime, anchored within a relatively stable world order. Initially, practitioners of comparative politics stressed variations on the theme of democratic stability that were central to the subfield in the 1960s and 1970s. As the Bundesrepublik matured and fears of democratic breakdown waned, research on Germany came to emphasize institutions, parties, evolving public opinion, and other issues associated with the study of stable democracies. The themes of normalcy and stability have survived the subsequent growth of political economy, albeit in somewhat different forms, such as corporatism's focus on class compromise and consensus. The editors also point out that the normalcy of the Bonn Republic was critical to the Cold War security system's structure and the associated emphasis on bipolarity in international relations during the Cold War era.

The editors believe that these perspectives have overemphasized structure and continuity in their explanations of German politics. These approaches are not well suited to the context of German unification, which embodies a break with the normalcy of the Bundesrepublik. Theoretical and substantive orientations that were heretofore dominant in the study of Germany are likely to decline in importance. This is especially true of theories in international relations that stress system structure. While some approaches, such as those in comparative politics focusing on the evolution of liberal democracy, remain useful in considering the implications of the new historical context, the editors maintain that they are not too helpful in identifying its causes: comparative history is better equipped for both tasks, especially the latter. The contributions to this volume are united by their consideration of continuity versus change, structure versus process, and the role of ideas and ideology in the study of German politics. After a review of past research on the Bundesrepublik and its extension to united Germany (parts 1 and 2), these themes are explored in four different substantive settings: "Political Economy," "Women, Politics and Society," "Inter-German Relations and the Politics of Unification," and "Germany and Its External Environment" (parts 3–6, respectively). Some sections of *From Bundesrepublik to Deutschland* are more unified and coherent than others, and there are differences in the clarity of the book's central themes, which emerge with the greatest lucidity in part 6.

The *Domestic Politics of German Unification* is more modest in scope. Eschewing explicit involvement in epistemological debates and evaluations of theoretical approaches, this volume centers on the substance of domestic politics before, during, and after German unification. Four key questions serve as the focal points of this collection: How did the process of unification take place and how can it be understood? What are the effects of unification, both short- and long-term, on the democratic process in Germany? What are the consequences of unification for German political institutions? What are some concrete examples of policy problems facing the united German? (p. 10). Thus, this collection centers on a number of issues that Huelshoff, Markovits, and Reich believe will recede in importance, although question one occasionally leads the authors onto similar terrain. When this occurs, they reach similar conclusions regarding the impact of the "undigested" legacies of German history.

The book is organized into two parts. Part 1 focuses on the process of unification and the politics surrounding it. With the exception of Lembruch's treatment of regime change in East Germany, the individual contributions center on parties and electoral politics. Part 2 focuses on institutions and policy alternatives. The emphasis in this part is on existing and potential institutional changes. Two recurring themes emerge: the impact of unification on essentially West German institutions (including the possibility of a new German constitution) and the problems of transferring these institutional arrangements to the eastern Länder.

Differences in approach make these books useful companions on a number of different dimension. At the level of larger themes and approaches to understanding Germany, *Domestic Politics* reinforces *From Bundesrepublik to Deutschland*'s argument that great attention should be given to the interplay of historical continuity and change, and that united Germany represents a synthesis of Germany's various pasts. *Domestic Politics* also reinforces the editors of *From Bundesrepublik to Deutschland*'s faith in continuing work on the evolution of German democracy as a useful social-scientific counterpart of historical analyses. The very insightful contributions found in *Domestic Politics* suggest that rather than receding behind concepts such as nationalism, national identity, and power, the study of Germany's domestic institutions is integral to understanding them.

These books also complement each other in terms of coverage. The sheer number and complexity of issues surrounding German unification make it impossible for a single volume to provide a comprehensive treatment. Out of necessity, the contributors to *Domestic Politics* do not address issues involving international politics. There are important insights to be gathered here, given the role of external forces in contributing to unification itself and lingering questions concerning Germany's role in world politics. This gap is closed, at least partially, by several of the contributions to *From Bundesrepublik to Deutschland*. For example, a chapter by Markovits and Reich and one by Huelshoff examine Germany's position within post–Cold War Europe. Their accounts provide solid support for the thesis that Germany is becoming an economic (rather than a military) hegemon within Europe and a key actor within a multipolar world. Furthermore, Markovits and Reich link Germany's relations with, and partial dominance of, Europe to key characteristics of its domestic politics, most importantly its commitment to liberal democracy and commercialism, which provide a more or less reluctant Germany with ideological leadership. Huelshoff's contribution also stresses domestic politics via his application of Putnam's two-level game metaphor to Germany's behavior within the European Community.

On the other hand, the coverage of domestic politics per se provided by *From Bundesrepublik to Deutschland* is rather weak—troubling because the book's subtitle implies a substantial treatment. Unfortunately, there is no sustained consideration of many of the core issues of German domestics politics, though certain elements of it, such as women and politics, the transformation of the eastern Länder, and the rise of the radical Right, receive considerable attention. The emphasis on parties, electoral politics, and institutional arrangements found in *Domestic Politics* fills these gaps admirably.

In addition to filling unavoidable gaps in each other's coverage, these books compliment each other in the areas where they overlap. In some cases, the overlap provides a more complete picture of issues that are fairly well understood. An example of this may be found in the fit between *From Bundesrepublik to Deutschland*'s historical analysis of the women's movement, East and West, and such contributions to *Domestic Politics* as Anderson's presentation of trends in the representation of women in the Bundestag and Kommers' consideration of the constitutional dimensions of the abortion issue. In other cases, overlaps between the two books remind us of just how tentative our understanding is and help to identify areas where a relatively greater amount of research will be needed. For example, the contributions by Allen (*From Bundesrepublik to Deutschland*) and Hancock (*Domestic Politics*) leave the reader uncertain as to whether macro- or mesocorporatism will constitute the dominant model of industrial relations in Germany, not to mention the possibility of a breakdown in the generally amicable relationship between business and labor. Of course, this is the case because industrial relations, like many other aspects of German politics, are still in a state of flux. However, suggestions and speculations regarding the likely alternatives provide future research with useful points of departure.

Obviously, it will be some time before German unity and its implications are properly understood. Each of these edited volumes makes solid individual contributions toward that end. Taken together, their contributions are even stronger.

Southern Methodist University W. DAVID PATTERSON

Soldiers and Politics in Eastern Europe, 1945–90: The Case of Hungary. By Zoltan D. Barany. New York: St. Martin's, 1993. 243p. $65.00.

This is one of the best of the "year 1989" studies of the complex relationship between the Communist party and the military organization. What characterized this relationship in Eastern European polities at this time was the effort to depoliticize the military.

The Party is the most combative superstructure of Soviet regimes. The military clearly is subordinated to a hegemonic Party that tolerates no political or ideological opposition. Yet the cleavages are internal—within the military and the Party, rather than between the two. Even if the military is the third structure of power (after the secret services and the Party), its elites are well represented in almost all levels of Party leadership.

What makes Eastern European armies even more subservient to the Party is the role played by the Soviet Army in "the transformation of the East European armies according to Moscow's desiderata" (p. 18). Barany takes us through all the stages of transformation that the Hungarian military underwent—from Sovietization (1945–53), to subordination (1953–68), to accommodation and recalcitrance (1968–88), to the final democratization (1988–90). Unfortunately, this is the weakest section of an otherwise well researched study.

The author is methodologically sound and has carefully followed the literature that dealt with civil–military relations in communist and totalitarian systems. Once again, the study confirms the hegemonic role of the Party over the state and the military.

In examining the utility of various models on civil-military relations in communist systems (e.g., Colton and Odom in Herspring and Volgyes's *Civil Military Relations in Communist Systems* [1978] and also Roman Kolkowicz's *Soviet Military and the Communist Party* [1967]), Barany finds they are "at best . . . clusters of generalizations and, for the most part, unsubstantiated hypotheses," since most relate to the Soviet model (p. 169). I find that even so far as the Soviet model is concerned, much of the "modeling" in the 1960s and 1970s bears no resemblance to what we know now about the Soviet military. The ethnic and regional divisions, as well as the nature of the relationship between the Party and the army, has been oversimplified.

What is most significant in this study is the author's painstaking effort to analyze the methods employed by the Party to gain and maintain control over the military institution. Of great interest is the final comparative chapter that deals with civil–military relations in Eastern Europe. Especially interesting is the analysis of the mechanism of party control over its armed forces in Hungary, Poland, and Bulgaria before 1990 and the adoption of the Soviet model as an instrument of its domination. What is unfortunately missing (although the publication date is 1993) is an explanation of the role and attitude of the military at the end of Party domination and, above all, whether the army split on, encouraged, or rejected reform.

Now that the reformers have been electorally defeated and Russia has become a rival and not an enforced ally, I wish the author would have elaborated on what portends for future civil–military relations in Eastern Europe.

This excellent comparative study of the role of the military in Eastern Europe (especially Hungary) confirms the subordinate role played by the military in communist countries. Barany confirms the view that the military did not act independently to forestall the democratic revolution of 1989 once its courses of authority collapsed. Barany's thorough and profound analysis clearly demonstrates that the military never acted as a political pressure group, as some prominent theorists of civil–military relations of communist countries (mainly the USSR) incorrectly claimed. This study, hopefully, will pave the way for new works, as well as new approaches, in the field of civil–military relations in former communist countries.

American University Amos Perlmutter

Hemmed In: Responses to Africa's Economic Decline. Edited by Thomas M. Callaghy and John Ravenhill. New York: Columbia University Press, 1994. 573p. $50.00 cloth, $18.50 paper.

The 1980s were a decade of economic crisis in sub-Saharan Africa. Beleaguered by stagnant growth and mounting debt, many countries embarked on World Bank and IMF-sponsored adjustment programs. Several years of erratic reform produced hesitant stabilization in several countries, but few signs of a robust return to growth. As the 1990s dawned, Africa was gripped by a second regional crisis, this time political in nature. Dozens of single-party states and military dictatorships, confronted by resurgent domestic opposition and international pressure, liberalized their polities or initiated transfers to competitive electoral rule. The wave of political transitions produced equally tentative and uneven results, ranging from democratic reform to civil war and state collapse.

There has been a dearth of analysis linking these epochal changes. This volume is an indispensable contribution, for it provides a wide-ranging, theoretically informed effort to address the complex issues of economic and political reform in the region. The studies collected here examine Africa's position in the global economy, the role of international financial institutions (IFIs) in guiding regional reform, and the economic and social impact of orthodox adjustment policies. Several authors also assess the political context of economic policy change, and the implications of political transition for future reform.

If there is a shared perspective among these writers, it is this: reform was inevitable for Africa, given the disastrous trajectory of economic decline in the early 1980s. Some of the central elements in the structural adjustment package (fiscal restraint, price reform) were necessary, though not sufficient for economic recovery. However, the orthodox agenda in Africa has been a substantial failure. This is partially attributable to the inherent limitations of underdeveloped markets and weak states, but it is also a consequence of global economic trends and the policy myopia of the IFIs. While Africa faces a limited array of options, feasible alterations in the domestic reform agenda and the international economy might significantly improve the region's discouraging prospects. Sustained recovery is also contingent upon a dramatic change in the political context of economic management.

The opening chapters examine Africa's experience with debt and adjustment. In his overview of the IFIs' approach to reform, John Ravenhill notes that the IMF and the World Bank have undergone a limited process of learning during the past decade. The Bank in particular has moved away from a simplistic adherence to "getting prices right" and stabilizing fiscal and external balances. By the end of the 1980s the Bank readily acknowledged the importance of domestic political reform, institutional development and international debt relief as essential components of economic recovery. They also called for more flexible implementation and appropriate policies to offset the social costs of adjustment. Despite the rhetorical shift, Ravenhill admonishes the IFIs for a doctrinaire insistence on price reforms and a rigid application of neoclassical principle to diverse African countries.

A more pointed criticism of orthodoxy is offered by Reginald Herbold Green. He stresses the inherent tension between the contractionary stabilization policies of the IMF and the ostensible growth objectives of Bank-supported structural adjustment. The Bank, though more adaptable than the Fund, has been excessively sanguine about the capacity of African economies to adjust rapidly in response to policy changes, and naive about the serious distributional and welfare costs of structural adjustment. Green notes that the alternatives proffered by African regional organizations are not very compelling, but he argues for a reform strategy which accords a greater role for state tutelage and productive public investment, a more variegated approach to adjustment, and an appreciation of the domestic political constraints on reform.

David Gordon and Matthew Martin focus on the central role of external debt in the African crisis. Gordon traces the genesis of the global debt crisis and its acute impact on African economies. Intractable debt obligations entailed a dominant regional role for the IFIs, who, as "lenders of last resort," provided resources linked to the adoption of orthodox reform. While recognizing the pervasive influence of IFI conditionality, Gordon refutes a monolithic view of these institutions. In the course of the 1980s, IMF and World Bank stipulations were often disparate, and the growing scope of conditional lending tended to fragment and dilute the impact of conditions. Most African regimes, through circumstance or design, failed to comply with important performance requirements, and the IFIs possessed limited leverage to enforce compliance. The result has been a pattern of partial and uneven reform, as weak institutional capacities and domestic political imperatives undermined commitment to externally-driven reform.

In a detailed comparison of Ghana and Zambia, Matthew Martin examines the negotiation process among governments and the IFIs. He finds inherent flaws in the way these institutions deal with debtor countries, noting especially the proliferation of complex and contradictory requirements attached to adjustment lending. Aggressive conditionality by the IFIs ultimately hindered sustainable reform, as governments adopted politically risky adjustment policies, without obtaining compensating external resources or witnessing dramatic economic improvement.

Turning to the political dimensions of the African crisis, Naomi Chazan and Donald Rothchild examine the effects of economic decline on state-society relations. The regional economic crisis was rooted in the predatory domination of weak authoritarian regimes. Economic stagnation paradoxically eroded the foundations of these regimes, thus aggravating state enfeeblement and political instability. Chazan and Rothchild discern a corresponding strengthening of civil society, as communities and social networks fashioned independent coping strategies amidst economic privation. Revitalized civil societies successfully challenged the *ancien regime* in numerous countries, yet the ultimate results of this struggle have been inconclusive. Throughout much of Africa, the absence of effective public authority is mirrored by a nascent yet fragmented civil society, which is unable to provide a coherent alternative to bankrupt states. This political impasse remains a hindrance to economic restructuring.

Taking up the pivotal relationship between political and economic reform, Thomas Callaghy challenges the guiding assumption of the IFIs and bilateral donors, who envisage a natural and positive synergy between economic liberalization and political democracy. He argues that successful economic reform is contingent upon the nature of institutions and the character of state power, rather than ideology or regime type. A pervasive "political logic" of regime maintenance in Africa has typically contravened the logic of economic rationality, and Callaghy does not foresee an easy resolution of this tension through the process of political transition. New democracies are especially vulnerable to distributive demands and rent-seeking pressures, and political liberalization could easily weaken state capacity to sustain reform. In an instructive comparison of Ghana, Nigeria and Senegal, Callaghy observes Ghana's adherence to economic liberalization under authoritarian rule; Nigeria's increasingly shaky (and ultimately unsuccessful) effort to pursue simultaneous economic adjustment and democratic transition; and Senegal's weak economic reforms under democratic auspices. The key distinguishing feature of these cases was the relative ability of political elites to insulate technocratic reformers within the state and to resist ubiquitous distributive claims from social groups.

Callaghy's comparative insights are underscored by Nicolas van de Walle, who provides a compelling account of "non-reform" under Cameroon's authoritarian regime. The central features of orthodox reform (fiscal compression, market liberalization, privatization) threatened the Biya regime's discretionary control over resources, a crucial pillar of neo-patrimonial rule. Pressed by domestic fiscal crisis and IFI conditionality, Biya's government temporized on key policies, placated donors to sustain a limited flow of external resources, and provided rents and sidepayments to regime supporters. Lacking both the commitment and institutional capability to seriously implement reform, the Cameroonian government manipulated adjustment measures to preserve the clientalist underpinnings of the state.

Michael Lofchie demurs from the latter authors, offering a more favorable evaluation of orthodox policies and the liberal reform scenario. In a comparison of Kenya and Tanzania, Lofchie notes that these countries virtually "traded places" in development strategy and performance during the past decade. Tanzania, a moderate socialist regime mired in economic distress, shifted from a strongly ideological orientation to a more pragmatic, liberalizing stance in the mid-1980s, with salutary effects. Kenya, for many years regarded as a successful bastion of African capitalism, experienced declining performance as the Moi regime undermined growth in the quest to reconstruct ethnic clientalist networks and bolster the basis of single-party dominance. Accepting the virtuous relationship between political liberalization and economic reform, Lofchie sees political pluralism as the key to better performance in both countries.

When examining the sectoral impact of structural adjustment in Africa, agriculture justifiably commands greatest attention. Three chapters respectively examine micro-level, meso-level, and macro-level dimensions of the rural sector. Focusing on the behavior of rural households, Sara Berry illuminates the problem of civil society in the countryside, and offers a skeptical assessment of donors' claims regarding the benefits of reform for agricultural growth. Observing that rural survival strategies center on diversification, Berry portrays the ways in which agricultural producers hedge their sources of income and seek access to resources through social networks. In circumstances of economic austerity, producers shift production and intensify off-farm employment, possibly lowering returns in labor and commodity markets and limiting long-term productive investment. Household economies in Africa do not engender a clear productive response to price reform. Moreover, commitments to kin groups and community networks tend to diffuse the elements of civil society.

The circumstances under which rural producers mobilize to defend their collective interests is a crucial issue taken up by Jennifer Widner. Many of the survival strategies cited by Berry were unavailable to cocoa producers in Cote d'Ivoire during the commodity crisis of the late 1980s. Unable to shift to new sources of income, or to draw on the resources of kinship networks, Ivorian villagers increasingly attributed declining rural conditions to government mismanagement, and voiced their grievances through organized dialogue and protest. The propensity to engage in "proto-politics" was influenced by the relative strength of traditional authority structures and state institutions in the villages, as well as the return of better-educated younger people from the urban areas. While highly significant, the exercise of political "voice" by rural producers did not signal the emergence of a coherent rural lobby in Cote d'Ivoire. Rural political initiatives were scattered and ad hoc, and villagers did not readily affiliate with opposition parties in the early 1990s.

Jeffrey Herbst sketches the general features of Africa's declining agricultural performance, and the leading reforms essential for sustained recovery. He then considers three countries which have seemingly avoided the "urban bias" typical of regional economic policies. In Kenya, Zimbabwe and Ghana, regimes fashioned programs comparatively supportive of agriculture. Herbst attributes these favorable policies to an idiosyncratic array of factors: in Kenya, the political salience of rural elites and the nature of the (formerly) single-party electoral system; in Zimbabwe, the historical influence of settler agriculture and the resolve of the Mugabe government to foster African farmers' organizations; and in Ghana, a recognition by the Rawlings regime that agricultural revival was essential for economic recovery. The dilemma, as Herbst rightly observes, is the limited transferability of these examples to other African states.

Citing the relative neglect of industrial strategy in the scheme of orthodox reform, Roger Riddell offers compelling evidence to demonstrate the detrimental effects of structural adjustment on manufacturing in Africa. He argues that wholesale, rapid liberalization of trade and exchange rates has battered African manufacturers and impeded the development of enhanced productive capacity. In contrast to the inefficient statism of the 1960s and 1970s, the IFIs have counterposed a strategy of "harsh withdrawal" with corrosive effects. What is needed, argues Riddell, is a "benign interventionist" approach which recognizes a role for government tutelage and protection, while also enhancing the competitive basis of manufacturing policy and fostering a stronger basis for productive investment.

Hemmed In is an ambitious work, and one might observe both shortcomings and excesses. There is a good deal of repetition here, and several chapters might have been trimmed of redundant material. Conversely, there is relatively uneven material on civil society and the social impact of adjustment, especially in the urban sector. The volume also lacks a comparative empirical overview of regional stabilization and adjustment experiences. A profile of this sort would provide a useful context for the cases which follow. Cavils aside, this collection offers the most important recent contribution to the literature on Africa's political economy. These chapters, rich in analytic insight, impressive in their thematic scope, and replete with important case materials, represent the best current work on the region. Indeed, the value of this collection extends well beyond its regional focus, as the authors address the political dynamics of economic reform, sectoral problems of structural adjustment, comparative political transitions, and important trends in the international economy. It will be of longstanding value for comparativists and students of international political economy.

American University PETER M. LEWIS

Politics, Feminism, and the Reformation of Gender. By Jenny Chapman. New York: Routledge, 1993. 315p. $79.95.

After years of neglect, the recruitment of women to public office has become a lively topic for scholars on both sides of the Atlantic. American scholars have focused on the structure of incentives and institutions affecting women's candidacy, whereas Europeans have examined the socioeconomic and partisan factors determining their presence among political elites. Jenny Chapman's research is in the latter tradition; but it raises issues about the interplay of social status, party organizations and gender roles that are relevant on both continents.

Chapman develops and tests a theory of recruitment to account for women's exclusion from elective office, and she places her analysis within a comparative and historical context: "A universal problem requires a universal explanation" (p. 8). Her broad perspective and extensive knowledge of diverse literatures provide a rich texture to her analysis of the complexities of female recruitment, although her ambitious approach also creates problems of organization. The book's great strength and most serious drawback, however, hinge on the difficulty of integrating traditional social science models of causation with feminist theory. Chapman's attempts to explain women's political status as a social and economic out-group in the first half of the book do not mesh comfortably with her views concerning their roles as caregivers in the second half. Thus, the contradictions in feminist thought that Chapman so carefully documents permeate her own analysis as well.

Chapman's ambivalence about the source of women's exclusion from public office is evident in her use of *scissors* as a metaphor for her concept of female recruitment and for her organization of the book. One blade of the scissors represents women's own occupational attributes, which consistently cause them to remain inactive or to be overlooked or eliminated by party selectors. The other blade reflects men's ability to construct society according to male gender roles, which inhibits women's entry into the public sphere.

Chapman analyzes the occupational bind for women—the first blade of the scissors—through an extensive survey of winning and losing candidates for local office during 1984 in Strathclyde, Scotland. On the ideological Right, she argues, women's increased educational attainment does not lead to the high professional status or the control of land and small businesses that characterize politically successful males. On the Left, where egalitarianism is embraced at least in principle and educational and social credentials are less important, women's absence from the industrial sector denies them participation in the dominant unions that promote low-status men into the political elite. Whether educated or not, then, women fail to fit the profile that dominates the major parties and end up being indistinguishable from losing men. Furthermore, this result holds for minor parties and independent candidacies in Strathclyde. The author bolsters these findings with comparisons from the United States and the former Soviet Union teased from case studies conducted primarily in the 1970s. Unfortunately, they are of limited value because of outmoded and inadequate data—a limitation Chapman herself raises. Consequently, the Strathclyde survey, with all its local idiosyncrasies and problems of scale, carries the burden of her argument.

To complement the modest empirical foundations of her analysis, Chapman turns to the reproductive role of women and its implications for the development of female leadership. This is the second blade of the scissors, but it actually has two cutting edges. The first is the male value system that subordinates women because it discounts their experiences and traits as mothers. In effect, Chapman argues, to be successful in the competitive, hierarchical world of men, women must reject the very qualities that define their gender. She illustrates this dilemma with a fascinating comparison of the political advancement of women in postindependence Finland and postrevolutionary Russia, which demonstrates again why neither education nor egalitarian ideology is sufficient foundation for women's entry into the political elite.

The added bite of this scissor's blade comes from the failure among feminists to recognized the inherent conflict between the private sphere of mothering and the public sphere of electioneering. Chapman charges that feminists' belief in equal rights and social democracy is misconceived and uses the experiences of the West German Greens, the Norwegian parliament, and the Women's party in Iceland to make her point. These are success stories in the sense of above-average representation for women as candidates and members of the legislature, but feminist organizations played only a small part in bringing them about. Instead, the author argues, each advance occurred because of conscious decisions by party activists to recruit women (even to the point of quotas, for the Greens) and by female candidates to stress their *differences* from men, rather than their similarities.

Although some might argue that Chapman distorts feminist thinking, this aspect of the argument has profound implications for women and for Chapman's own analysis. It suggests that efforts to recruit more women to legislative office will ultimately fail if they depend on women's becoming more like men in terms of credentials. Furthermore, it indicates that women will be unable to compete for political office unless women's work as caregivers is recognized and compensated through a reordering of social values. In short, men and male roles are what need changing before women will be able to assume their share of responsibilities for democratic governance.

Chapman's conclusion is not only profoundly pessimistic but also raises questions about the relevance of the first half of her book. Her argument that women are excluded from office not by discrimination but by the lack of attributes characteristic of politically successful men loses much of its force when juxtaposed with her assertion that women's role places them "outside" the entire structure of economic and political life. In the end, she suggests that it is not economics or social status that bars women from political office but gender as it is constructed by men.

Political scientists, accustomed to looking for causation among economic, social, or structural variables, may find this conclusion difficult to embrace, particularly since it demands a cultural revolution of unprecedented scale to achieve political parity between men and women. Whether or not the reader is persuaded by Chapman's arguments, there is much to ponder here: the distorting effects of traditional, education-based measures of status when applied to men and women; the historical ebb-and-flow of women's struggle for equality; the mediating effects of parties in promoting women into leadership roles; and the influence of ideas on political competition.

Syracuse University LINDA L. FOWLER

The Nation and Its Fragments: Colonial and Postcolonial Histories. By Partha Chatterjee. Princeton: Princeton University Press, 1993. 282p. $49.50 cloth, $15.95 paper.

Religious Nationalism: Hindus and Muslims in India. By Peter van der Veer. Berkeley: University of California Press, 1994. 247p. $40.00 cloth, $14.00 paper.

One of the major trends of this decade is the rise of new movements for religious nationalism in such places as the Middle East, Northern Africa, Central Asia, and South Asia. Their encounter with the secular nationalism of Europe and America has led to what Samuel Huntington has called "the clash of civilizations," or what might be called the beginning of a "new Cold War."

The two books under review are important contributions to the understanding of this encounter. In both cases, they give insights into the pivotal role played by culture in the current efforts to redefine national identity in India, their major point of reference. In Chatterjee's analysis, India is the prime example of how the "colonial and postcolonial histories" experienced by a people lead to new, albeit fragmented, national identities. In Veer's book, case studies of the rise of Hindu and Muslim nationalism in the Indian subcontinent provide ways of understanding how culture-based politics fit into the modern world.

Because India looms so large in both of these books, they may not get the attention they deserve: one may be tempted to shelve them in an "area studies" category reserved only for Indianists. This would be a great mistake, for their attempts to present a non-Western perspective on world politics are of universal interest. Both books are written in an engaging style and should be read by anyone concerned about the fate of the contemporary world, perhaps especially by those least knowledgeable about non-Western cultures.

Partha Chatterjee is one of India's leading political philosophers and a well-known member of the subaltern school—a group of scholars, many of them Indian, who have applied Gramsci's notion of nonelite, or "subaltern," views of society to the historical and contemporary political experience of formerly colonial countries such as India. Fundamental to their scholarly enterprise is the attempt to resuscitate the perspectives of peoples who have been oppressed or marginalized due to gender, class, caste, and local frames of reference.

In an influential previous book, *Nationalist Thought and the Colonial World,* Chatterjee claims that nationalism of the type that the British bestowed on India was a "derivative discourse." According to Chatterjee, the secular nationalism of Britain and America, which was so admired by Congress leaders such as Nehru (but not Gandhi, who had a more traditional vision of Indian nationalism), is a political vestige of Enlightenment ideology. Increasingly, this paradigm has been questioned by postmodern theorists and challenged by local political leaders.

In his latest book, *The Nation and Its Fragments,* Chatterjee presents a series of essays that advance this line of thinking, and address the question of how a noncolonial form of nationalism is possible in postcolonial countries such as India. He argues that the construction of an authentic nationalism—one rooted in the experiences of a people—requires an appreciation of the fact that the social whole has, in countries such as India, been seen only from fragmented perspectives and that there has been no single national culture.

Chatterjee observes that the absence of an authentic Indian nationalism is, to a large extent, the result of colonial rule, which denied to Indians the ability to develop unified economic and political centers of power. Although the British presence brought the infrastructure of unity in the form of trains, roads, and an administrative bureaucracy, it was in the interest of the colonial powers to keep the social divisions of caste, class, religion, and region intact. Only a small westernized elite in India experienced a sense of being part of a social whole; and by taking over the economic and political systems that the British developed, this elite stratum of Indian national leadership simply replicated colonial patterns.

Yet although India remained colonial in its business and political practices, Chatterjee argues, it has developed its own independence in the spiritual and cultural spheres, which the British were largely unsuccessful in altering. For that reason, the search for an authentic postcolonial nationalism begins with culture: "Here nationalism launches its most powerful, creative, and historically significant project: to fashion a 'modern' national culture that is nevertheless not Western" (p. 6).

At least at the beginning, however, this national culture is to be found in a fragmented form, as various groups within society give voice to their own distinctive experiences. It is for this reason, presumably, that India has undergone such violent tensions of competing cultural nationalisms: Hindu, Muslim, and Sikh.

To understand these religious rivalries in their recent manifestations, one should turn to Peter van der Veer's insightful *Religious Nationalism: Hindus and Muslims in India.* Like Chatterjee, Veer regards secular nationalism as an example of the "Western discourse of modernity" that is profoundly rejected by religious nationalists in places such as India. In turning to their own traditions for a new sense of national identity, these new political activists necessarily build on traditional cultural ties to Hinduism, Sikhism, and Islam. This does not mean, however, that these cultural nationalisms are ancient or entrenched. In their current form, they are rather recent inventions, products of the political searching of the present age. For that reason, Veer sees them as possibly transitional steps toward an emerging, panethnic, indigenous nationalism.

The recent conflict at the Indian pilgrimage city of Ayodhya is the book's central case. In recent years, nationalist Hindus have yearned to destroy Ayodhya's old mosque, built by the Moghul emperor Babar on the site of what many have come to believe is the birthplace of the God Rama and replace it with a Hindu temple. The Hindu political party, the Bharatiya Janata party (BJP) based much of its support on its ability to champion this demand; and when an unruly mob finally did destroy the mosque on 6 December 1992, the BJP was blamed for the action. In Veer's perceptive analysis, Ayodhya becomes a metaphor for religious nationalism, where in Veer's frame of reference, the control over the definition of public space is a central element.

To the credit of both of these authors, they are able to deal dispassionately with a subject that usually generates more heat than light. In attempting to reserve judgment about the ultimate value of religious nationalism, they will undoubtedly be criticized by those who adopt what Veer describes as the usual "liberal" position of comdemning religious politics out of hand. The extremists among these critics—the "new Cold War" hawks—will claim that Veer and Chatterjee have been duped by the religious facists in the BJP. Yet neither of the authors are directly supportive of any particular group of religious nationalists. In fact, their analyses could be used to criticise those leaders of the BJP and other movements who exploit for their own personal self-interests the legitimate popular desire to see cultural values enshrined in public pronouncements.

These are important books for understanding the cultural and political changes of the contemporary world, especially that part of it formerly known as the Third World. In their quite different but equally insightful ways, Veer and Chatterjee have helped to blaze the trail for a scholarly assessment of what will increasingly become a salient feature of postcolonial politics—the emergence of nationalistic ideologies and political theories based on local cultural ties.

University of California, Santa Barbara MARK JUERGENSMEYER

Democracy in the Caribbean: Political, Economic, and Social Perspectives. Edited by Jorge I. Domínguez, Robert A. Pastor, and R. Delisle Worrell. Baltimore: Johns Hopkins University Press, 1993. 312p. $48.00 cloth, $14.95 paper.

Democracy in the Caribbean: Myths and Realities. Edited by Carlene J. Edie. Westport: Praeger, 1994. 320p. $65.00.

Two identically titled books on democracy in the Caribbean within a year is either a remarkable coincidence or evidence of rising scholarly and policymaker concern, or both.

There is indeed cause for concern. However vigorously some may celebrate functioning democratic systems in the English- and Spanish-speaking Caribbean (Dominican Republic and Puerto Rico) and signs in the French-speaking (Haiti) and Dutch-speaking (Suriname) areas that democracy under siege may yet return, the danger signs are plentiful: declining economies, declining international transfers (including remittances), and rising un- and underemployment may combine with inability of ruling parties (related to the above) to dispense the patronage that has bought them electoral support. The prospect: instability and violence. Furthermore, Guyana and Trinidad-Tobago have shown a proclivity for injecting race into electoral politics, dividing their societies along communal lines at the same time as Apartheid was being dismantled in South Africa. And Cuba remains staunchly authoritarian. These are only a few of the cloud banks that are massed on the horizon. All these issues are ably dissected in both books.

These two volumes round out a half-dozen Caribbean-wide analyses in English in an equal number of years since *The Caribbean After Grenada: Conflict, Revolution, and Democracy* (1988) by MacDonald, Sandstrom and Goodwin. Caribbeanists have been blessed with *The Poor and the Powerless: Economic Policy and Change in the Caribbean* (1988) by Thomas, *Conflict, Peace and Development in the Caribbean* (1991) edited by Beruff, Figueroa, and Greene, *Society and Politics in the Caribbean* (1991) edited by Clarke, and *Storm Signals: Structural Adjustment and Development Alternatives in the Caribbean* (1991) by McAfee.

This is quite extraordinary, multi-focused, and multidisciplinary for an area not known for drawing such attention from scholars. One of the books here reviewed (the Edie volume) fits with the rest in being written by academic social scientists, happily including some junior faculty. The Domínguez volume is divided roughly in half between academics (mostly senior people) and policy analysts with institutional affiliations. Both focus on democracy. However, with some qualification it is probably fair to say that the Domínguez authors are content with conventional western notions of liberal democracy while contributors to the Edie volume exhibit more skepticism and critical analysis.

Since all definitions are arbitrarily true, it may seem trivial to discuss them. However, as all Caribbeanists know, there is little agreement among them on what makes sense to include under the "Caribbean" rubric, not to mention treatises on democracy in the region. The Domínguez volume rules out a Caribbean Basin definition as too heterogeneous, including as it does "Central America, Mexico, Colombia, Venezuela, and, indeed, the United States" (p. xi). Exclusive focus on the Commonwealth Caribbean, on the other hand, is seen as too narrow, as it leaves out Cuba, Puerto Rico, the Dominican Republic, and Haiti. Neither book deals with Associated States such as the other Francophone societies, Aruba and the Dutch Antilles, or with dependencies such as Anguilla, Bermuda, or the British and American Virgins presumably because one cannot properly speak of democracy in a non-self-governing territory. Yet both books contain chapters on Puerto Rico.

While we are on definitions, the following amalgam of formal or institutional criteria to define democracy represents the consensus between the books: regular, free and fair elections offering meaningful competition for government power, without use of force, through wide popular participation (universal suffrage and nonproscription of parties), with guarantees of civil and political rights (free speech, association, petition, and religion), plus an independent judiciary and respect for the rule of law. Evelyne Huber, in the Domínguez volume, emphasizes that substantive political and socioeconomic equality does not necessarily flow from fulfillment of the formal criteria.

Edie's volume goes beyond these criteria by including social justice, and emphasis on *participatory* democracy and mobilization to balance the typical Caribbean pattern of educated middle class control of the state through clientelism and racial politics. Here the difference in emphasis and tone between the books comes out most clearly. Edie's (and Hintzen's) stress on control by the bourgeoisie of the less educated masses as one of the chief legacies of the Westminster parliamentary model is countered by Domínguez (and Payne) focusing on its utility as a defense against authoritarianism. Both views have merit.

The books operate on a different scale. The Edie volume seeks to contribute to better understanding of Caribbean democracy, and perhaps to a theory of democracy in developing nations. It certainly achieves the first goal. The attainment of the second is debatable, especially since so little theory is discussed. The contributing authors for the most part remain content with spelling out the particulars of their assigned nation case study without, regrettably, even looking across the boundaries for illuminating comparisons. Thus the most that can be said is that the pieces may serve as a data base for someone to weave together more general propositions.

Ironically, the best theoretical contribution (beyond the Domínguez introduction, about which more presently) is Evelyne Huber's "The Future of Democracy in the Caribbean" in Domínguez et al. That book is offered as an amalgam between the two typical books on the Caribbean: those aimed primarily at specialists (which certainly seems to fit the Edie volume), and those addressed to crafters of a new Caribbean policy in the international system.

The Edie volume, however, geared so much more than Domínguez et al. toward detailed case analyses of internal politics, provides some of the more trenchant policy insights. For instance, we may extrapolate from Noguera's analysis of democratization and foreign intervention in Grenada, Nicaragua and Panama that "restoring democracy" in Haiti through external intervention is not likely to be successful.

Each book attempts more than the other, in different senses. The Domínguez volume presents more general analyses of problems such as drugs, corruption and violence, and economic performance. The Edie volume seeks comprehensiveness in scope: only four of fifteen chapters (beyond the introduction) are general commentaries. The rest are case studies, including Suriname, Cuba, and Haiti—the latter explicitly eschewed by Domínguez et al. because of their isolation from the rest of the region and the uncertainty about their future, as well as Cuba's experiment with statism which has "proven itself no more effective . . . than elsewhere . . ." (p. xii).

Given the special prominence in people's consciousness of Cuba and Haiti, the rationale of the sponsors, publishers and editors of the *Perspectives* volume for excluding them is both regrettable and rather thin, and particularly surprising in view of Domínguez's well-known expertise on Cuba. Even more surprising is the absence of commentary on Cuba in the general-topic chapters where, except for brief mentions of its independence, only negative foci draw attention, e.g., the trial of highly placed Cuban authorities for corruption and drug trafficking (p. 50–51), U.S. security concerns due to the pro-Cuban stance of Nicaragua and Grenada after 1979 (p. 111), and, ironically, "Cuba is too big to ignore" (p. 274). Remarkably, the only reference to Fidel Castro in the Domínguez et al. volume is a clever wisecrack about Cuban change awaiting "either a heart attack or a change of heart" (p. 274). Without questioning the sincerity of the rationale for exclusion one might be forgiven for wondering what role ideological disposition had in the decision.

By contrast, Edie's volume includes excellent chapters on Suriname, Cuba, and Haiti. Betty Sedoc-Dahlberg's piece on Suriname is one of the best in the book. She is a seasoned and courageous analyst operating under extremely delicate conditions. Kenneth Boodhoo is no less so. His chapter on Haiti offers depth and perspective in these days of agonizing debate over international options for stemming human rights abuses and restoring the democratically elected Father Jean-Bertrand Aristide to power. Carollee Benglesdorf's chapter on Cuba is perceptive and balanced, offering keen insight into the post-Gorbachev (post-Perestroika) and post-Cold War restructurings of Cuba's economy and even party. She provides helpful perspective on the Fourth Party Congress in 1991 by comparing its discussion of decentralization and participation with the 1970s discussions of these issues. She concludes that "Decentralization is not a synonym for democratization, nor, for that matter, is increasing participation. . . . Indeed, no form of nascent opposition outside the party will be tolerated. The leadership's harsh . . . treatment of the tiny human rights groups . . . underscores its confiscation of the political arena" (p. 193–194). This is fine analysis, and extremely helpful in understanding the region. One hopes Domínguez et al. and Johns Hopkins will reconsider the Cuban exclusion for the second edition—by which time perhaps the original rationale will have disappeared anyway.

The best feature of either book is the Domínguez introduction. It is a masterful survey, genuinely comparative not only within the Caribbean, but also with South America, Africa and Europe. It systematically sets forth the gist of the contributions that follow while placing them in a contextual and conceptual framework. Not only does it relate Caribbean socioeconomic conditions across the board, but it gives the reader a comprehensive perspective on possible explanations. This is a model of what an introduction to a book on comparative politics of a region should be. It alone is worth the price of the book.

Domínguez's best contribution is perhaps his discussion of the "statist bargain," beholden to Keynes, according to which "democracies delivered material gain" with the state serving as midwife: "economic growth gave birth to social welfare," typically in the form of patronage for the party faithful. Capitalism and democracy married, with labor gaining rising living standards through accepting profits, markets and political expression, while business paid the price of accepting the welfare state in order to buy peace. Foreign consumers and governments helped pay the price in the days of decent demand and prices for Caribbean export commodities and Cold War driven aid generosity. But those days are no more. The state can no longer afford the statist bargain. Whether liberal democracy can survive its end, says Domínguez, "could be the Caribbean Question for the 1990s" (pp. 11–12, 22). At the other end of the book Robert Pastor and Richard Fletcher, in an edited version of their *Foreign Affairs* (1991) article, take up that policy question and deliver a set of recommendations that may in the future be seen as the post the Caribbean states rounded on their way from economic decline and threatened instability to peace and relative plenty. Remarkably comprehensive, the plan calls for "self-reliance that is outward-, not inward-, oriented," with U.S. support in terms of opening markets to exports, "perhaps as part of a broader extension of a North American Free Trade Area" (p. 257). The book thus begins and ends with major contributions. That the material flanked by these offerings has a hard time chinning up to that level is hardly surprising.

The issues of security and future instability emerge in several forms in both books. The Domínguez volume, being more (U.S.) policy oriented, expresses more extensive concern. Such a discussion is not standard operating procedure in comparative politics, but entirely appropriate in a region characterized by small states in a strategically sensitive location that has drawn unwelcome present and past U.S. and European external dominance.

The books come up with different answers on invasion for democracy. Domínguez sees what is now clearly "a pattern of international behavior in this subsystem" of intervention "on democracy's side" as more than good luck (p. 18). Edie, citing Noguera's chapter on U.S. interventions in Grenada, Nicaragua and Panama, asks rhetorically, "Can democracy still continue to be a convenient pretext for the United States to extend its power in the Caribbean" (p. 5)? (Although the books do not, and could not, address a Haiti invasion since they were published before that became an option, we may infer from Noguera's analysis that "restoring democracy" in Haiti through invasion is not likely to be successful. He points to a "rapid decline in U.S. interest toward countries it has invaded or destabilized" [p. 228].)

Anthony Maingot, in the Domínguez volume, makes a major contribution by arguing that drugs and corruption present the most severe security threat to Caribbean mini-states that appear more concerned about preserving "sovereignty" than about coordinated international action against what clearly is an internationally coordinated challenge. Such a redefinition of security in this region is long overdue.

There are several regrettable features about these two good books.

(1) The Edie volume occasionally sounds intolerant in not giving the benefit of the doubt to fellow scholars' or political practitioners' intellectual integrity. Thus, for instance, mobilization is seen as "thinly veiled attempts to camouflage the reality of middle-class power" (Hintzen, p. 20). The "mere existence of the formal mechanisms of democracy such as political parties, periodic elections, and representative bodies" is contrasted with "genuine representation" (Edie, p. 11) in a way that appears to assume the non-genuineness of the practitioners of the formal mechanisms. And scholars like Samuel Huntington are lambasted for having produced "accommodating reformulations," creating the

modernization approach emphasizing order rather than democracy when dictators in LDCs turned out to be pro-Western in the Cold War (Hintzen, p. 9–10). Interestingly, considering this attack, Domínguez cites his Harvard colleague positively (p. 6).

Huntington's early work *Political Order in Changing Societies* (1968), the usual focus of his detractors, may indeed have overstated the priority of stability and order. However, let us acknowledge its brilliance and assume his sincerity in trying to find answers to extremely complex problems while noting that all of us are prone to modifying our thinking according to our milieu, our understanding of the world, and our ideology. Those who are prone to leftist social critique are doubly vulnerable to the temptation of "vanguardism," with which we now feel comfortable charging the New Jewel Movement. Caribbean scholars who exhibit such neocolonial traits should be embarrassed.

(2) It is shocking that neither book contains a map of the region, this despite Richard Bloomfield's assertion in the Foreword to the Domínguez volume that "the same characteristic that has made the Caribbean important to the United States in times past—its geography—will make it impossible to ignore in the future . . ." (p. viii). All comparative politics books should enable the reader to lay a spatial foundation for comprehending the subject. Needless to say, that applies at least as much to IR books.

(3) For whom are the books intended? Apparently fellow academics, graduate students, and policy makers. There seems to be slim concern with accommodating the undergraduate student or general reader, especially in the Edie volume, where unnecessarily complex language is used by Edie herself in her Jamaica chapter and by Hintzen in his Trinidad and Tobago study. Vaughan Lewis in the Domínguez volume also excessively obfuscates and complicates his subject. By contrast, it is sheer pleasure to read the prose of Franklin Knight in the same volume.

(4) Finally, an interesting set of questions about the role of the state are raised by implication in these books, but not answered. Is there a relationship among (a) the (vanishing) statist bargain, (b) the perceived need to protect the sovereignty of mini-states from international domination through coordinated action against drug cartels, and (c) the unwillingness of analysts to raise questions about the assumption that the traditional state model is still the most relevant and appropriate one for the twenty-first century? Euraque and Niemann's pioneering "Regional Economic Integration in the Periphery: A Comparison of Central America and Southern Africa, 1870–1990" (*Southern African Perspectives* 31, 1994) calls upon us to raise such questions about the proper role of the state, to explore regional solutions and, accordingly, not to accept sovereignty incantations at face value. Those of us with more than passing professional interest in the Caribbean have some work to do in this realm.

Having said all that, it is well to reiterate: these are good books that should be wisely read. In their own way, and especially together, they contribute significantly to our understanding of democracy in the Caribbean, democracy in general, and the Caribbean in general. That is no small feat.

University of Hartford HARALD M. SANDSTRÖM

The Elite Connection: Problems and Potential of Western Democracy. By Eva Etzioni-Halevy. Cambridge: Blackwell, 1993. 239p. $39.95 cloth, $19.95 paper.

Etzioni-Halevy intends *The Elite Connection* as a text or supplement for political science and sociology courses, as a contribution to democratic elite theory, and as a warning about threats to democracy. The first four chapters review and criticize many theories of democratic politics and assert the advantages of democratic elite theory. The next two chapters present Etzioni-Halevy's *demo-elite perspective*, stressing "relative elite autonomy" from domination by other elites, especially state elites, as the overarching metaprinciple of democracy and highlighting the egalitarian potential of elite autonomy. The last four chapters apply the demo-elite perspective to a number of empirical cases: democratic stabilization in Britain, democratic breakdown in Weimar Germany, the recent democratic transitions in Poland and Russia, and recent politics in Western democracies illustrating both the threats to democracy by elite actions that reduce autonomy—"subjugation" (e.g., union bashing) and "collusion" (e.g., government–business coziness)—and the egalitarian possibilities stemming from social movements and established elites' accommodating responses.

For students, the book provides a reasonably comprehensive overview and critique of social-scientific theories of democracy and a succinct statement of the demo-elite perspective. However, undergraduates would find these six chapters opaque, because the discussions are rarely tied to empirical examples. By the time empirical cases are discussed in the last four chapters, the typical student will have drowned in a sea of metatheory, and the cases are often treated so briefly that most undergraduates would need supplementary information. Graduate students, on the other hand, should find the literature review useful (though not definitive) and the theoretical explication and applications thought-provoking.

As a scholarly contribution, the book makes some important points that others have made but that need repeating and developing. Etzioni-Halevy persuasively challenges negative images of elite theory, asserting that its presumed flaws are not inherent, especially not in her demo-elite version. Elites are not viewed as superior beings: they simply have great power and influence because they command resources—coercive, material, organizational, symbolic, and "psycho-personal" (e.g., charisma). Elites include leaders of protest movements, which gives the theory an egalitarian bent. Elites are not omnipotent; they must contend with "subelites" and the "public," which have significant influence in a democracy. Moreover, Etzioni-Halevy highlights the vital point that most competing theories erroneously attribute motives and actions to abstractions (classes, interest groups, organizations, civil society). Elite theory's distinct advantage is that its actors are real people.

Etzioni-Halevy locates her core idea of elite autonomy in the liberal concept of the separation of powers and in the works of Weber, Mosca, Schumpeter, and Aron, which emphasize countervailing elite power as the foundation of democracy. Their mistake was failing to challenge economic inequality, which her demo-elite perspective avowedly corrects but actually says little about. Elites (and subelites) are "relatively" autonomous (never absolutely) when their resources are not controlled by elites outside their boundaries. Relative autonomy is evidenced mainly by elites confronting other elites—for example, by working against their interests or undermining their legitimacy. Etzioni-Halevy tries to distinguish her approach from those emphasizing elite consensus on "rules of the game," arguing that elite cooperation is more important to democracy and that cooperation rests primarily in elites' self-interested desire to preserve the system. Still, she admits that "elite and sub-elite cooperation in a democratic regime may well require some consensus about the desirability of democracy through free elections and civil liberties as such" (p. 110).

Many scholars accept that democratic stability depends on all major elite factions' feeling secure from attack by a hegemonically inclined elite. Relative autonomy helps describe this condition. However, I doubt that making it the core concept is a theoretical advancement. For one thing, Etzioni-Halevy's main empirical indicators of autonomy are rather vague. In reading them, I kept thinking that Western democratic elites are frequently less autonomous—more interdependent—than these descriptions suggest; and I do not think this typically amounts to subjugation or collusion. Military elites (the clearest example) are substantially less autonomous than her abstract descriptions imply. The military also illustrates difficulties with her interest-based conception of autonomy: many analysts think the general elite commitment to values requiring military subordination to civilians constitutes a major reason for democratic stability; but in distinguishing her contribution from previous works, Etzioni-Halevy unconvincingly downplays elite values.

Furthermore, while she argues persuasively that elite autonomy or its absence helps explain Britain's successful democratization, Germany's democratic breakdown, and democracy's greater promise in Poland than in Russia, she does not say why autonomy emerged in one context and not another. She comes closest in comparing Britain and Germany, stressing the "different stature of the state" (p. 148). After the sixteenth century, British state power was gradually checked, which did not happen in Germany. But one still wants to know why and why not. That requires probing deeper into elite structure and values than Etzioni-Halevy seems willing to go.

Additionally, her analysis of elite autonomy in contemporary Western democracies contains some troubling elements. She sees established elites' autonomy as weakened in the 1980s by instances of subjugation and collusion, making democracy less secure. While giving plausible examples, she lacks evidence that these more seriously threaten elite autonomy than past instances. She then argues that the proliferation of social movements in Western democracies since the 1960s and established elites' cooptative responses have made these democracies more dynamic, more flexible, more egalitarian—seemingly contradicting her "warning" that democracy is less secure today. A final difficulty with Etzioni-Halevy's emphasis on elite autonomy is that while acknowledging the need for elite cooperation, she mainly sees it as threatening autonomy. Yet a fundamental problem for Western democracies today is that elites are so bent on advancing their autonomy and interests that they cannot cooperate to serve the common good.

Despite these shortcomings, Etzioni-Halevy manages to steer rather clearly through some exceedingly murky waters and ultimately makes a good case that relative elite autonomy is at least an intuitively useful concept for thinking about the foundations of stable democracy.

Loyola College, Maryland MICHAEL G. BURTON

Politician's Dilemma: Building State Capacity in Latin America. By Barbara Geddes. Berkeley: University of California Press, 1994. 246p. $35.00.

Barbara Geddes's book is an ambitious attempt to apply collective action and rational choice arguments to efforts at reforming the state in Latin America. In particular, she is concerned with explaining why state reform takes place under some circumstances and is thwarted under others.

This is a significant question given the importance the state has assumed in Latin American development since the 1930s, and also given the growing conviction that a healthy state is a sine qua non for successful development strategies. Yet this question has not been systematically explored, notwithstanding the existence of some good monographs on individual countries. Indeed, in general the state has been a poorly researched topic despite its importance. Thus a first key contribution of this book is the search for a systematic comparative answer to a major research question.

Geddes assumes that 1) under democracy, whether state reform occurs depends primarily on politicians and 2) politicians are self-interested rational actors, primarily concerned with their own reelection. Therefore, the viability of state reform depends primarily on the extent to which it is in politicians' self-interests.

The dilemma from which the title is taken is that on the one hand, politicians depend on selective incentives (patronage) to build up their political constituencies and enhance their chances for reelection. On the other hand, an effective state must be somewhat insulated from politicians in order to promote development. How, then, will politicians behave? Will they thwart or support civil service reform and other measures that would presumably strengthen the state but reduce patronage opportunities?

Geddes convincingly claims that looking at this issue as a collective action problem leads to largely pessimistic conclusions regarding prospects for state reform. Nevertheless, as the game theoretic literature on cooperation suggests, cooperation for a better collective outcome (i.e., state reform) is not impossible. These broad arguments build in intelligent fashion upon the works on collective action and cooperation. Geddes then devotes one lengthy chapter to the Brazilian case (pp. 43–82), which underscores the contentions about the difficulties of state reform while also pointing to specific cases of effective bureaucratic insulation.

Much of the rest of the book argues that institutional arrangements and political contexts determine whether rational politicians would be more or less likely to support state reform (especially civil service reform). Two chapters look at the logic of legislators in supporting or thwarting state reform. Beginning from a prisoner's dilemma model, Geddes deduces that a majority of politicians will support reform only if there is a situation of relative parity among the parties. Given such parity, the members of different parties will more or less equally lose patronage opportunities and thereby avoid losing a comparative advantage over electoral competitors, while simultaneously reaping the small benefit gained from supporting state reform. Geddes then tests this model with data from Colombia, Uruguay, Venezuela, Chile, and Brazil.

This argument on why legislators support or oppose state reform is problematic for three reasons. First, Geddes assumes that the benefit of supporting reform (*e* in her model) is quite small. But if *e* increases in value, then members of the majority party or coalition would support reform even in a situation of non-parity. So the conclusion that reform will be supported only in situations of relative parity rests on assuming that *e* is low.

It seems likely that *e* rose considerably relative to the historic pattern with the crisis of the state in most Latin American countries in the 1980s and early 1990s. Far reaching state reform occurred under democracy in Argentina (1989–present) and Bolivia (1985–89). Less sweeping but still significant reform occurred in Venezuela (1988–93), Brazil (1990–92), and Peru (1990–92). None of these cases approximated the conditions Geddes stipulated as favorable or necessary for state reform. Stated in more prosaic terms, it is possible that broad regional trends are more important than parity among parties in shaping prospects for state reform.

Second, the model is predicated on the assumption that politicians' only interest is (re)election. Granted, this assumption is made in the name of parsimony and simplification, but there are good reasons to wonder how reasonable it is for many Latin American cases, and therefore whether a model based on that assumption would prove compelling. In Brazil, for example, 30% of incumbent deputies regularly do not seek reelection, making it hard to claim that reelection is their motivation for policy positions.

Third, Geddes assumes that access to patronage is crucial for most politicians' chances of winning reelection. As an assumption made in the name of parsimony, this makes sense for Colombia, and to some degree Brazil and Chile. In Colombia, party organizations have little control over the nomination process, and election depends primarily on each politician's linkages to constituencies. Because of the open list system in Brazil, politicians of the catch-all parties typically rely extensively on patronage. Chile also has an open list, providing an incentive for politicians to develop their own clienteles via patronage. But relatively disciplined and centralized parties have controlled the nomination process, making politicians also dependent on party elites. Venezuela until recently had a closed list system for the lower chamber, so reelection depended more on a politician's position on the electoral list—and hence on her/his good relationships with national or provincial party bosses—than on patronage. Uruguay also has a closed list system, albeit one in which dozens of party factions present their own list, making candidates dependent on faction leaders for their reelection. Moreover, television has increasingly displaced patronage as a factor in senate elections in Brazil, Chile, and elsewhere. In brief, the extent to which politicians depend on patronage for election in these five countries varies considerably according to institutional arrangements—as Geddes herself powerfully argues elsewhere in the book.

After examining the logic of legislators contemplating state reform, Geddes also analyzes presidents' appointment strategies. Do presidents primarily make appointments to achieve administrative efficiency or to satisfy "hungry parties?" She argues that presidents' strategies depend on features of their political environment. More specifically, presidents who are creating their own party vehicles are likely to concentrate appointments among party and personal loyalists, with less regard for administrative competence. Presidents from well organized parties generally prefer to maintain personal control over appointments rather than delegating them to the party. Finally, presidents who won the nomination despite the opposition of most party leaders, or who are independents, are likely to make appointments on the basis of personal loyalty or merit and to eschew party based nominations.

Geddes argues persuasively that three factors affect presidents' ability to follow their preferred appointment strategies: the legislative strength of their party, the degree of party discipline, and the amount of competition the president faces in her/his own party. The micrologic she ascribes to rational actors is generally compelling, and she presents empirical evidence showing that her model is quite powerful.

Although many parts of the argument are questionable, this is an important and intelligent book on a timely subject. It is among the best rational choice works on Latin American politics.

University of Notre Dame SCOTT MAINWARING

The Portuguese Military and the State: Rethinking Transitions in Europe and Latin America. By Lawrence S. Graham. Boulder: Westview, 1993. 153p. $34.95 paper.

As usual, U.S. policy in Latin America and many of the scholars who write about the area have it wrong—in this case concerning the role of the military institution, civil–military relations, and the strategy to be used to help consolidate transitions to democracy. Americans, both scholars and policymakers, often tend to have a myopic, wishful, and ethnocentric view of Latin American—and, perhaps by extension, southern and eastern European—civil–military relations in which the U.S. system is the model and all other countries must try to emulate the U.S. pattern. But of course, the U.S. model of strict subordination of military authority to civilian leadership is not the only model of civil–military relations (even in the United States, not the most prevalent form) and probably not exportable. It is the strength of Lawrence Graham's new book on the Portuguese military that not only does he point out the inadequacies of the U.S. model for countries where the level of development and the institutional setting are far different, but he also shows how the Portuguese case may constitute an alternative model especially suited for the similarly fragmented countries of Latin America and Eastern Europe.

At least two major problems come to mind in thinking about the relevance of the American military model to Latin America. First, outside of the Southern Cone (Argentina, Chile, Uruguay), there tend to be immense social, cultural, economic, and racial gaps between the military officer corps and the civilian elites. The civilians, mainly white, tend to look down on the military as "peasants with guns," useful in constituting a political alliance but never to be entertained in one's home.

Meanwhile the officer corps, usually mestizo or mulatto, often disdains the haughty civilian elites and seeks to use the military career as a route to power and wealth—which usually flow from seizing the national palace. Where a U.S. model of civil–military relations fits in this charged, social-racial context is often hard to discern.

The second aspect of this dilemma is institutional, and that is where Graham's book makes its major contribution; for the fact is that in southern and eastern Europe and Latin America, the military constitutes a separate institution, almost a fourth branch of government. While enjoying its own special rights and responsibilities in a corporate and quasi-feudal sense, it is also integrated with the state and with political parties and interest associations in many and complex ways. To expect that these complexities can be subsumed under the simplistic slogans of "military subordination to civilian authority" or "educating the Latin militaries to their proper role" is naive and ignorant of historical or current realities.

Our expectations in these regards have been raised by the apparent successes of the Argentine, Chilean, and Spanish cases in subordinating (for the most part) the military to democratically elected governments. But as Graham points out, these societies are relatively homogeneous and prosperous and reached a widespread consensus on the goals to be achieved. In more fragmented societies, however, which are ethnically and socially stratified and often lack consensus on ends or means (the majority in Latin America and Eastern Europe), this model may not apply—hence Graham's argument that for *most* countries seeking to make the transition to democracy, the Portuguese case may be more relevant than any others.

Graham portrays Portugal since the 1974 revolution that destroyed the old Salazar regime as a highly fragmented society with a variety of groups—civilian and military, often overlapping—competing for political power. It is not sufficient, Graham says, to comprehend Portugal only in terms of the reconstitution of civil society and the emergence of civilian leaders and political parties with democratic values. Instead, one must also understand, empathetically, the military as a prime and independent actor, as a main national institution, and as a catalyst for change. In addition, one must view the state not just as a unitary institution but as a vast bureaucratic structure with distinct civilian and military "households."

Graham is masterful at tracing the different military and civilian factions in Portugal since 1974, how they overlap as well as separate, their interpenetration, and their virtually constant negotiations and accommodations. Elections in this context were like opinion polls: they showed the direction of popular sentiment and offered some legitimacy to the winner, but the results were often viewed as tentative and as offering only a partial mandate. Of greater or at least equal importance were the factional disputes that involved both civilian and military elites, the personalistic rivalries, and the series of tentative but always evolving political pacts that represented each new adjustment of the balance among them. Meanwhile, a gradual evolution toward a more consolidated democracy and greater civilian control did occur. But note also the tentative language in which the accomplishments are expressed, since the military–civilian (and other) negotiations are still ongoing.

Clearly this process and complexity cannot be subsumed under the too-simple notion of military subservience to civilian authority. Note, too, how close is Graham's model of a "bureaucratic state" constantly adjusting, accommodating, coopting, and balancing new elements to the model of "multiple power contenders" first set forth by Charles Anderson in the 1960s and to the "corporative" or "organic state" models of the 1970s. Graham has rediscovered some old truths, but his statement of them is nonetheless vigorous and original and adds detail and insight to our understanding.

Nevertheless, Graham fails to slay the whole dragon. While highly critical of the standard, U.S.-based system and model of civil–military relations in terms of its applicability to other nations and culture areas, his conclusion backs away from fully elaborating the new and innovative model that his own analysis would seem to suggest. One final criticism of this otherwise extremely significant book needs mentioning, and that is that it is often stiffly written and sometimes too elliptically argued. One almost needs to know Graham's own research journey (Brazil, Portugal, Angola, Mexico, Romania, Yugoslavia) and his previous writings to appreciate fully the arguments he makes here.

University of Massachusetts HOWARD J. WIARDA

Orpheus and Power: The *Movimento Negro* of Rio de Janeiro and São Paulo, Brazil, 1945–1988. By Michael George Hanchard. Princeton: Princeton University Press, 1994. 203p. $29.95.

While long a staple in discussions among political scientists working on the United States, race has not figured prominently within the work done on Latin America by political scientists. This is a gap that Michael George Hanchard seeks to fill in *Orpheus and Power.* Quite appropriately, the central question Hanchard addresses is why no Afro-Brazilian social movement of national scope developed in Brazil in the post-World War II period. To answer this question the author adopts a Gramscian framework, which is favored on the basis of its ability to emphasize and grasp the cultural dimension of racial politics. After introducing the book's conceptual framework and presenting an overview of the debate on Brazilian racial politics in Chapters 1 and 2, the author starts to present his argument. The lack of a social movement among Afro-Brazilians, Hanchard states, is due to the "racial hegemony" embodied in the ideology of Brazilian "racial democracy" that began to take root in the early part of the nineteenth century. Racial hegemony, by promoting "discrimination while simultaneously denying its existence" effectively neutralized racial identification among Afro-Brazilians and made mass mobilization unlikely (p. 6).

To develop this argument the author first traces, in chapter 3, the origins of the notion of racial democracy, turning primarily to the writings of authors such as Gilberto Freyre. In the next three chapters more empirically-oriented material is presented. The author draws upon archival material and interviews, particularly with activists from Rio de Janeiro and São Paulo. Because of the lack of any phenomenon that could be called a social movement of national scope, this book lacks the unifying focus the civil rights movement, for example, provides U.S.-based studies on race. Instead, a variety of incidents from everyday life, as well as the potentially most consequential manifestations of Black identity, are considered. The material is cogently presented and the story persuasively developed. The author shows how the vision of racial democracy impeded recognition of racially specific problems, which in turn undermined group solidarity around race and race-based mobilization of national scope.

This accomplishment notwithstanding, there remain many loose ends of a conceptual nature in this book. Hanchard may be right in stating that hegemony is a useful concept in that it allows one to grasp the cultural dimension of racial politics. Group identity, indeed, can be thought of as a precondition for collective action. But, as the author himself acknowledges, "the collective identification with a particular group or collectivity is an insufficient basis for political mobilization" (p. 98). A theory of collective action, in other words, involves more than the issue of collective identity. Recognizing this point, the author points to the need to look beyond the cultural dimension of racial politics and to consider a "broader social totality" and see "cultural practices" as "part of *whole social processes*—at once ideological, cultural and material" (p. 98, 139). Unfortunately, this sort of cryptic comment, never really spelled out and clarified, does little to shed light on a complex conceptual problem. The link between group identity and collective action thus remains untheorized.

What is most glaringly absent is a concerted effort to consider the link between national politics and potential social movements. In other words, there is no sustained attempt to consider the strategic dilemmas presented for incipient social movements by different patterns of national politics, a factor closely linked to the likelihood of social movements being formed. To be fair, reference is made to actions being "informed" or not by the "new logic of the *abertura*," that is, the political opening whereby Brazil moved from military rule to democracy starting in the mid- to late-1970s (p. 150). But such comments are made only in passing and do not form part of a theoretically guided consideration. There is simply no attempt to review and draw upon the insights that the voluminous works on transitions from authoritarian rule and democratic consolidation have produced and to consider how they could be incorporated into the study of race relations.

The main theoretical problem of *Orpheus and Power*, then, is that it focuses primarily on cultural politics. If institutional politics is of little importance to the study of race relations because, as Hanchard states, "much of Brazilian racial politics occurs outside of channels of representative democracy," this is something to be considered and assessed (p. 25, 57). And such an assessment can only be made if institutional politics is not brought in in an ad hoc fashion but as part of a conceptual framework that seriously addresses this dimension of politics. While this may be a theoretically challenging enterprise, there certainly is no good argument for the a priori rejection of the insight developed in various bodies of literature that stress how political institutions play a role in defining how groups become structured and relate to one another.

Such a theoretical critique does not mean that we must start from scratch, because this line of thinking is developed within the recent theoretical literature on social movements. Indeed, this body of literature, developed by sociologists as well as political scientists such as Sidney Tarrow, has clearly emphasized the need to conceptualize the linkage between national politics and the strategic choices faced by potential social movements. This literature, then, seems to be the natural frame of reference for a study that purports to analyze social movements in Brazil. But the entire body of literature on social movements is summarily dismissed (p. 157–58). The problem for Hanchard is not that he is opposed to cross-disciplinary work, as his advocacy of a "political-anthropological" approach indicates (p. 27). Hanchard's rejection of social movement theory, rather, is based on the lack of attention in this literature to the specific problem of race. This depiction of the social movement literature is accurate, to a certain extent, but to fully reject the relevance of social movement theory to race-based social movements entails a fundamental error: to assume what must be shown. If racially based social movements operate in a different manner than gender based or ecological social movements, this is something to be established through a general theory of social movements. In other words, the distinction between race and gender, or race and ethnicity, is something to be established through theoretically guided work, not through pre-theoretical assertions. Until this has been done, there is no reason not to see different social movements as sharing some basic characteristics inasmuch as they all seek to challenge the status quo.

Because of these shortcomings, this reviewer would characterize Hanchard's effort to fill the race gap in political science work on Latin America as one that meets only mixed success. If Hanchard rightly points out that Alfred Stepan's (1989) edited volume *Democratizing Brazil* could be blamed for not touching upon the impact of race on the process of democratization, his own work in effect replicates the mistakes of those he criticizes and does nothing to build bridges between mainstream political concerns and the study of race relations (p. 8). In some sense, then, we are back where we started: rather than a Latin American-based political science that ignores race we are given a study that focuses on race to the exclusion of the dynamics of democratization. This failure to engage the work done by political scientists on the regional move toward democracy is not fatal, but it does limit the likely impact of *Orpheus and Power*. Most damagingly, it is unlikely that it will affect the democratization debate to any great extent. Its significance, then, will be restricted to its contribution to the debate on Brazilian race relations and the comparative study of race relations. In this sense, Hanchard provides a worthwhile addition to the revisionist historiography that has questioned and sought to dispel the image held in the literature for some time that Brazil was a racial democracy. Countering this image, the author shows how the notion of racial democracy has prevented the politicization of racial issues and a frontal attack on the problem of racial discrimination.

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GERARDO L. MUNCK

The Health of Nations: Public Opinion and the Making of American and British Health Policy. By Lawrence R. Jacobs. Ithaca: Cornell University Press, 1993. 259p. $34.50.

The national debate on President Clinton's health care plan has once again put the spotlight on America's unique mix of welfare state policies. Unlike other advanced industrial countries, America has no national health insurance and its income maintenance and social service benefits are comparatively much lower. The underlying reasons for America's distinctive welfare state is a source of long-standing debate within political science. Some scholars suggest that America's liberal political culture fosters a distrust of the state which makes major government social welfare initiatives very difficult. This cultural explanation of politics and welfare state differences has been challenged by state-centered theorists who contend that state administrative capacity and government officials play key roles in shaping welfare state policies. In this view, America's fragmented, "weak" administrative structure is a serious obstacle to sweeping policy change and reform, especially in the face of well-mobilized groups antagonistic to the welfare state. By contrast, many European countries with their large centralized bureaucracies are capable of acting independent of societal interests.

In his excellent, timely book, Jacobs develops an innovative theoretical framework which builds upon the cultural and state-centered theories. His book is a careful, comparative study of two historic health care reforms—the British National Health Service Act of 1945 and the Medicare Act of 1965 establishing America's health insurance program for the elderly. He suggests that state-centered theorists (whom he terms Weberians) cannot adequately explain the British or American experience with these two major health care reforms. While British policymakers did nationalize the entire health care delivery system, they made major concessions to key interest groups such as the British Medical Association even though Britain had a powerful health ministry. In the United States, policymakers often resisted the demands of influential medical producer organizations, despite the weak administrative capacity of the national health bureaucracy. Further, Medicare represented a significant expansion of government's role in health care.

For Jacobs, the key determinant of the outcome of these two health care reforms was public opinion rather than state capacity. In Britain, the citizenry had learned to trust the state through decades of progressively more extensive government involvement in social welfare. Initially, British policymakers resisted the idea of a government-run health system and universal coverage. But public opinion pushed policymakers in this direction during the war years. With the landslide Labor electoral victory of 1945, the government had the political opportunity to respond to public opinion through the creation of the National Health Service.

In the United States, most citizens were highly skeptical of a major government role in health and social policy. Throughout the 19th century and the early 20th century, the federal government's involvement in social and health care policy was quite limited. Public social programs at the state and local level were viewed as inferior and reserved for the indigent. Jacobs observes that policymakers in the 1930s and 1940s failed to enact comprehensive health care reform because it was out of touch with the citizen's distrust of the state. In the late 1950s and 1960s, politicians responded to public opinion with a modified reform package, restricting benefits to the aged and emphasizing private administration. Johnson's landslide victory of 1964 gave him the votes he needed to push the Medicare legislation through Congress.

In this book, public opinion represents a country's political culture which evolves over a period of time as citizens interact with political institutions and private organizations. Yet Jacobs does not simply regard policy as the "epiphenomenon of culture" (p. 227). Instead, policymakers' reactions to pubic opinion are filtered through policy networks. Powerful producer groups had privileged access to government officials which affected the content of British and American health legislation. Elections profoundly affected the timing and character of legislation. Political culture, in other words, does not predetermine policy outcomes. Politics is about contest and different groups vying for power. Policymakers are sensitive to public opinion but the give-and-take of the political process means that political culture does not influence policy outcomes in a straightforward fashion.

Jacob's book is a valuable contribution to political science for three important reasons. First, his analysis is a creative re-thinking of prevailing theories of welfare state development. As he points out, comparative research on the welfare state has tended to neglect public opinion or use rather simplistic notions of political culture. His book introduces a sophisticated model of the relationship between public opinion, political institutions, and policy outcomes that will be of great interest and utility to future researchers.

Second, his discussion of public opinion is relevant to our understanding of contemporary domestic policy. Some theories of American politics suggest that public opinion sets the broad parameters of policy debate, leaving elite policymakers broad discretion to decide the content of policy. With this latitude, policymakers may be highly responsive to well-organized producer groups, even if the goals of producers are at variance with popular sentiment. Jacobs provides a needed corrective to this view. He suggests that for highly salient issues with substantial citizen interest, policymakers are frequently able to resist even fierce producer group pressure. Producers' groups are more likely to be successful on issues characterized by citizen apathy or disinterest. In these situations, elite policymakers are unlikely to see a political risk in making concessions. This dynamic has recently been in evidence in high profile issues such as drug policy whereupon Congress has tended to disregard political pressure from professional and service provider associations.

Third, the book contributes to our understanding of health policy formation. In this century, several failed attempts have been made to enact universal government health insurance in the United States. We are, as of this writing, trying once again. Jacobs' compelling, detailed research offers insights into the pitfalls and dilemmas facing contemporary policymakers as they try to craft politically feasible health care reform.

Jacobs' analysis is bound to spark healthy debate within political science. Skeptics might argue that the sweeping National Health Service Act stands in stark contrast to the Medicare Act which left the American health care system essentially intact and limited government insurance benefits to the elderly. Would a larger, more powerful health bureaucracy at the national level have fostered a more comprehensive solution to health care?

Also, the current contrast between Britain and the United States in health care is quite striking. In the last few years, Britain has instituted far-ranging—some would say radical—changes in its health care system, closing large hospitals, introducing market competition among hospitals, and limiting the power of some producer groups. This restructuring of health services has been implemented by the Conservative government despite broad public unease and the concerted opposition of the Labor party. How can we account for the relative speed and comprehensiveness of the British reforms? In the United States, we struggle to enact universal health coverage even with substantial public support. A plethora of health care reform proposals are pending in Congress, making the prospects for the enactment of any legislation very uncertain. How is public opinion mediated by key political institutions to shape the various bills and the public debate on health care?

The Health of Nations should be required reading for anyone interested in answering these questions. More generally, this important book should be on the bookshelf of any scholar interested in the development of the British and American welfare state, comparative politics, and the impact of public opinion and political culture on public policy.

Duke University STEVEN RATHGEB SMITH

Language Repertoires and State Construction in Africa. By David D. Laitin. New York: Cambridge University Press, 1992. 205p. $42.95.

David Laitin is a comparativist of remarkable breadth of intellectual scope and research experience. Relatively few scholars carry out field inquiry in more than one world region, and hardly any in so many diverse sites as Laitin has explored. Somalia, Kenya, Nigeria, Ghana, Spain, India, Estonia, Bashkortostan: skilled trackers can spot Laitin footprints in all of these strikingly different locales.

Equally diverse are his conceptual sources: Wittgenstein, Gramsci, Weber, and Geertz have supplied important theoretical resources for his earlier books (*Politics, Language and Thought: The Somali Experience*, 1977, and *Hegemony and Culture*, 1986). In the present work, he makes creative and effective use of game and rational choice theory, and draws extensively upon sociolinguistic research. the result is an elegant and compelling analysis of language politics and policy choice in Africa, by a long distance the most comprehensive undertaken in any field.

Laitin examines language use patterns throughout the African continent, demonstrating the dialectic between state policy choice and language market forces. But the comparative African analysis is constantly enriched by insights drawn from other settings, both those bearing intriguing similarities (India), and those radically different (Switzerland, Japan). Extended vignettes offer informed summaries of language processes in a number of countries (Morocco, Ghana, Nigeria, Zaire, Central African Republic, Kenya, Zambia, Zimbabwe). In short, the book—although of brief compass—is remarkably broad in its coverage.

The core thesis—entirely persuasive—is that the *longue duree* linguistic equilibrium in most of Africa is what Laitin labels the 3+/−1 outcome. This means a stable multilingual society, with a hierarchy of speech domains. The language of the former colonizer is likely to retain its status for many governmental purposes, higher levels of education, and as a medium of elite communication. One or more lingua francae will occupy an intermediate (or perhaps co-equal) position, be officially recognized as "national" languages, be promoted in the educational system (except at the university level), be used for some official purposes, and be praised in populist discourse. Many (though not all) vernaculars will hold their own, remaining as vehicles for local, kin milieu and household communication, often used in the first years of education. For those dwelling outside their area of origin, social necessity may require acquisition of the local vernacular; those who speak the lingua franca as mother tongue will only need command of two languages: thus the Laitin +/−.

Many language planning activists would have it otherwise; African language specialists and populist politicians have often entered impassioned briefs for elevating an indigenous language to exclusive national status. Laitin astutely deploys game theory to demonstrate why such efforts are almost certain to abort, as they did in India. State policy choices are important, and certainly have an effect. However, any policy prescription which overlooks the social marketplace is doomed to failure, and indeed may be counterproductive (as illustrated by the fiasco of the Sekou Toure effort to impose six Guinean languages as official tongues to be enforced through the educational system). Strategic calculus of the ordinary citizen enters the picture; what language repertoires are important for social mobility and economic advantage? And why should one not expect to acquire three languages (more or less), if this is the "normal" pattern?

State language policies tend to be burdened with the premise of a monolingual natural end point, one of the axioms of "national integration" as sacred vocation of the contemporary state. Many historic states have indeed largely accomplished this; Italy, at the moment of unification, had no more the 3% of its populace which spoke the official version of Italian, and no more than 10% understood it. But "successful" language policies are not necessarily transferable across world historical time and space.

Nor, Laitin argues, is there any reason to assume that the 3+/−1 formula is politically risky. Indeed, its greatest virtue is that it recognizes and endorses a reality produced by a citizenry unobtrusively registering in the social marketplace its own consumer preferences. Perhaps the greatest threat to its stability might come from active recognition and promotion of local vernaculars in preference to the lingua francae at the local level; one sees some trace of this in Nigeria and Ethiopia, where one hears claims in language and other spheres that public goods belonged only to "indigenes" of the state or region, and that "strangers" from other parts of the country should depart.

In the use made of game theory, Laitin acknowledges the important limits of rational choice concepts. He assumes disequilibrium to be a natural condition, and recognizes the primordial aspects of language as a component of social identity. He recognizes as well the social construction dynamic in language processes and politics. In freeing game theory from the shackles of unrealistic simplifying assumptions, which so often mar the work of its practitioners, he makes of it a genuinely useful analytical instrument.

In a mere 164 pages of text, Laitin sketches an amazingly comprehensive portrait. In its scope and succinctness, this comparative study is a model. More fundamentally, the volume is a masterful analysis of one of the most important aspects of identity politics in Africa and elsewhere. It is must reading for all students of African politics, as well as those concerned with the comparative politics of cultural pluralism.

University of Wisconsin, Madison CRAWFORD YOUNG

The Sources of Social Power, Volume 2: The Rise of Classes and Nation–States, 1760–1914. By Michael Mann. New York: Cambridge University Press, 1993. 826p. $79.95 cloth, $24.95 paper.

Michael Mann has produced a second massive tome in what is now to be a four-volume series on the sources of social power. Unlike the last volume, which ranged from "the beginning" to 1760, this one covers a mere 154 years—the "long nineteenth century" of "mostly peace," bracketed at the beginning by the Seven Years' War and at the end by the World War I. Unlike the first volume, which was largely noncomparative, in this one, Mann explicitly compares social and state development in France, Britain, Habsburg Austria, Prussia–Germany, and the United States. And unlike the last one, which was limited by the nature of the available source material, this one draws relentlessly on both statistical and qualitative sources. Not the least of Mann's achievements is his ability to gather and analyze comparable statistical material for much of this period for all five states.

Mann's basic preoccupations are the same as they were in volume 1; but apart from occasional cross-references, *The Rise of Classes and Nation–States* can be read on its own. However, the book will be difficult to read without a basic grounding in both the classical Western social theorists and their more recent successors: Moore and Skocpol, Dahl and Tilly, Giddens and Poggi, Rueschmeyer and Stephens—even, at times, Foucault. The book is also made more difficult (its formidable length and complexity aside) because of Mann's maddening proclivity for adding new terms to the jargon of comparative macro history, and using others in apparently personal ways.

Nearly every chapter of *The Rise of Classes and Nation–States* is larded with typologies and outlines. But its historical depth and empirical richness reward the effort it takes to comprehend them. Mann has a consummate ability to blend historical monographs and works of theorized history and historicized theory with his own insights and experience. He has apparently read almost everything written in English and French (though not in German) on these big subjects written over the past 30 years, synthesizing their authors' views into his own formulations. He does gloss over or miss some important historical debates (e.g., the emergence of markets in revolutionary America); but although this will leave some historians cross, it may be a good thing in a work of historical synthesis. This is a book to sweat over and argue with; it is not a book to ignore.

Not only that, but *The Rise of Classes and Nation–States* is refreshingly lacking in the obfuscating simplifications that are characteristic of the social theories of our decade. In fact, its tone and ambition are resonant of the 1970s, when Mann began writing it, though its problematic is more enduring. What are the sources of social power, and how do they relate to one another? Are one or more of them primary in structuring society? How did the development of these societies relate to one another during the periods in which their major classes and states arose? How did their different paths of structural development affect how they related to one another (e.g., in explaining the outbreak of World War I and the collapse of the long nineteenth century)?

The insights and observations produced by Mann's fecund mind as it ranges over a century-and-a-half for five major countries are legion, but several stand out. His readers will be prepared for his familiar fourfold typology of power (ideological, economic, military, political), one type being permanently primary. For Mann, the forms of power are not like bowling balls, but are *entwined* (a favorite term). At times, one form reaches out beyond the others (military power in the late eighteenth century) with a powerful effect on states and classes. But the forms of power are not "pure," not ranged against one another in a permanent game of primacy for the benefit of social theorists. The characteristic structural developments of the century emerged from their entwinings. Mann's abiding theoretical instinct is the systematic attack on all forms of determinism.

Nevertheless, states and their *crystallizations* turn out to have a shaping force on classes and nations that the other forms of power normally lack. This is in part because Mann, like Tilly, is impressed with the power that war has had to transform states and thus to lead them to transform their societies. States made war, and war remade states, deeply affecting the classes and nations they represented. War is also the major way in which these states related to one another, a theme Mann chooses not to develop fully until his final chapter on World War I.

Why this predominance of political power over other forms of power? In his crucial central chapters on the rise of the modern state, Mann declares himself an "institutional statist" (chap. 3). Only by specifying the institutional–political differences between states, he argues, can we adequately explain the dramatic differences in national development—and in class conflict—that he finds in his period. Thus, economic power, reinforced by ideological power, produce historical impulses; but they need to be translated by political and state power to understand the shape of the final outcomes.

The effects of Mann's institutional statism are seen most dramatically when he turns to the effects of industrialization on the West. "Capitalism and industrialism have both been overrated," he declares roundly in his conclusion. Their diffused powers exceeded their authoritative powers; and although they enormously increased collective capacities, they had remarkably little effect on distributive powers, which were largely the same at the end of the period as at the beginning. The varying outcomes of capitalism and industrialism from country to country were determined not by nations' stocks of resources, comparative economic advantages, or greater or lesser entrepreneurial endowments but by "authoritative political crystallizations that had been mostly institutionalized rather earlier," for example, by states (p. 726).

Students of comparative political economy may find Mann's conclusions about the industrial similarity of countries ranging from Habsburg Austria to the United

States somewhat hasty. His reasoning about the causes of state differences will also leave many readers unsatisfied. One in particular will raise many an eyebrow, namely, that territorial particularities are enhanced by agrarian economies but diminished by industrial ones (pp. 726–27). But for those interested primarily in social theory, the most interesting problem is how the statism in Mann's historical chapters meshes with the antideterminism in his theoretical instincts. For although he misses few opportunities for determinist-bashing, the final instances in his historical narratives often turn out to be the state and the effects of war and of war's side-products upon it.

But these are issues to argue about, not to dismiss. Although the book's length and complexity, personalized jargon, and innumerable typologies will not make it an easy classroom tool, especially for undergraduates, Mann packs so much historical and comparative synthesis into his book and is so widely knowledgeable about social and state development in Europe and America that no student of comparative or historical sociology or state building will want to ignore it. At least, I hope to be around when Michael Mann gets around to giving us his final two volumes.

Cornell University SIDNEY TARROW

Politics in the Portuguese Empire: The State, Industry, and Cotton, 1926–1974. By M. Anne Pitcher. New York: Oxford University Press, 1993. 322p. $62.00.

Substantive treatments of Portugal's empire since its demise in 1975 are rare occurrences. While there is an extensive literature on Portuguese Africa during the 1960s and the first half of the 1970s, at the high point of the conflict between guerrilla movements pushing for independence and a recalcitrant authoritarian government in Lisbon refusing to cede any territory, studies of Portuguese colonialism since have been infrequent or, when published, partial and incomplete. Given this context, Anne Pitcher has advanced our knowledge of the dynamics of the Portuguese empire and its interrelationship with Salazar's and Caetano's New State regime significantly.

In taking the case of the Portuguese cotton industry and cotton production in the colonies, Pitcher has selected an arena that provides excellent insight into the dynamics between the state and groups and films in society engaged in economic activities. As a consequence, she clarifies a number of crucial dimensions relevant to understanding not only how Portuguese colonialism operated but also the policy instruments of the New State and its impact on politics, society, and the economy. First of all, she establishes the differences in these interrelationships according to whether one is looking at an authoritarian regime in the making (1926–1936), at a consolidated authoritarian regime (1936–1946), at an authoritarian regime undergoing significant transformation (1946–1958), or at an authoritarian regime in decline (1958–1974). Second, she shows how the regime was shaped by the need to build a broad coalition pulling together a disparate collection of antidemocratic forces in order to consolidate its power, how it was corporate in nature really only during the fascist era in Continental European politics, and how corporatism once again became incidental to regime dynamics as political leaders tried to transform an economic and political order in decline. In contrast to those who look at Caetano's rule as an era of reform and change, she characterizes the *caetanato* as one full of irreconcilable tensions and contradictions that ultimately led to the collapse of Portuguese colonialism and authoritarian rule. Third, the interplay between broad-scale regime dynamics and the operations of the cotton industry and trade introduces a degree of specificity into our understanding of the political economy of authoritarian Portugal that has been absent hitherto in Portuguese studies.

But this book is more than just an isolated case study of Portuguese authoritarianism or colonialism. It contributes to our understanding of how a strong state operating under authoritarian precepts shaped an economy and society according to the policy preferences of its leader. It examines the policy legacies of that state in the economic stagnation later of firms seeking to modernize and the costs entailed to the nation in generating a divided industry and the unresolved dilemmas which subsequently would have to be faced by a democratic regime seeking to integrate its citizens into a wider concept of European community.

The book is carefully researched and well-documented. Its particular strength is its clarification of how, despite the monolithic pretensions of the New State, authoritarian rule in Portugal was comprised of a complex set of competing forces and interests and how the state Salazar created sought to mediate and control that conflict by retaining the upper hand. Through engaging in precise analysis of cotton interests. Pitcher brings into focus the dynamics of authoritarian Portugal. With these lenses, she demonstrates why this regime was far from static and how it was continually undergoing change and evolution through Salazar's determination to retain control over the mainland and the regime's overseas extensions.

My only criticism concerns what she characterizes as the final stage of Portuguese authoritarianism (1958–1974). Although Pitcher captures regime formation and consolidation exceedingly well, I am not convinced by her argument that it is correct to characterize this regime as one in decline for its last 16 years. What is certain however, from the data and material she amasses, is that the cotton industry and trade were in decline during these years. It is here that this case study reaches its limitations, in attempting to generalize about a whole regime and era from a single set of actors and circumstances. For, without a broader perspective on the state and the economy during the 1960s and early 1970s, what is missing is a balanced assessment of the attempt to transform state and society by reformers and the crucial role played by the country's leaders once Salazar was incapacitated. Since Spain underwent significant transformation from within during the 1960s and early 1970s, what needs clarification is why Portugal failed to accomplish similar change. My own view is that the explanation is to be found more in individual leaders, in who emerged as the successors to Salazar, and their policy choices, rather than in systemic arguments as to the inevitability of the Portuguese collapse as a consequence of structural conditions and factors.

University of Texas, Austin LAWRENCE S. GRAHAM

Vietnam: The Politics of Bureaucratic Socialism. By Gareth Porter. Ithaca: Cornell University Press, 1993. 227p. $29.95.

This is essentially a historically oriented study of Hanoi's post–Vietnam War institutions and its state and Party socioeconomic policies, with a brief concluding chapter on foreign policy. In the later years of the Vietnam War, the author, a long-time Vietnam specialist and academic activist, was codirector of the Indochina Resource Center in Washington, which at the time amounted to the country's chief antiwar lobby. The book is divided into seven chapters. There is an introduction by George Kahin, Cornell's guru of Southeast Asian studies, under whom Porter studied as a graduate student.

Today is not the most propitious moment to be publishing full-length studies of contemporary Vietnam, so kaleidoscopic is the scene since the momentous decisions of the Vietnamese Communist party's Seventh Congress in mid–1991. After a decade-and-a-half of stagnation, Vietnam suddenly finds itself thrown into future shock. What were mere clouds on the horizon at the time this manuscript was closed have now become torrential downpours.

The first of this mandated change is the massive Party effort to restore its lustrous image of the wartime years. Indeed, in those days, the Party (i.e., chiefly its cadre structure, perhaps 10% of the total membership) was an impressive political institution, holding a reputation among rank-and-file North Vietnamese as being almost superhuman. Cadres had satisfactory answers for every question; they were more competent than anyone else on the scene; they were incorruptible; they were an ideal role model. But battlefield attrition and then time itself began to take their toll. The war ended, and cadre morale began to sag; performance declined; corruption crept in, along with rising "mandarinism" (i.e., selfish, imperious misuse of office).

Leadership rectification efforts (endless self-criticism sessions and semipurges) were too cautiously applied out of deference to past loyalties and service. Recently, however, came a more ruthless weeding out of the incompetent, not vindictively but simply to get them out of the system and into harmless sinecures. At the same time, an intensive nationwide recruitment drive has been launched to earmark the young for future top leadership assignments. One Hanoi official described this as the Party "scouring the boondocks for the best and brightest young." The concepts behind Party renovation are (1) that the Party is not as discredited as some of the communist parties in East Europe; (2) that the Party is, and must be, committed (better, *resigned*) to eventual political pluralism in Vietnam (in the form of the now much detested "multiparty system"); and (3) that if it renovates itself well and coopts the most promising of the country's future leaders, it can as a political party take on other political challengers (by fragmentation and playing one faction off another) so as to win political control at the polls, openly and honestly. This is a bold but risky course, but the Party just might be able to pull it off in, say, about five years.

The second "dated" quality about this work has to do with Vietnam's economic sector. This is not a thesis book as such. Rather, it is descriptive of the events that have buffeted "socialism Vietnam-style." Unlike many writers, the author does not sharply separate Marx from Lenin—that is, Marxian economic theory, historical interpretation, and philosophy from Leninist instruments of governance to organize, mobilize, and motivate a society. This is just as well; for the question of just how Marxist Ho Chi Minh and his fellow rulers were (clearly, they were true Leninists) grows more uncertain with increased transparency of the history of Hanoi's internal political dynamics in those wartime years. It is to be noted that Vietnamese media and spokesmen today employ more variety of "official" terms for the economic system being put in place (*market mechanism, market orientation, logic of the situation,* etc.), but never *socialism.* There is great semantic ambiguity here, and it is deliberate.

Vietnam's contemporary economy is hardly addressed by the author. There is a brief and generally satisfactory account of the early years leading up to 1991, treating the problem of poverty, systemic inability to save and invest, and a decade of extraordinarily poor Party–state economic guidance. Yet even here the author fails to note the basic debate between industrial-oriented export strategy and closed-door protectionist strategy. Unfortunately, the writing cuts off the moment when Vietnam actually began moving away from a command economy and towards a market economy. The heart of politics of today's bureaucratic socialism in Vietnam is found here, between the Politburo-level factions who genuinely mean to allow the logic of the situation (as it is delicately put) to fix economic policy and those who wish only to tinker with the machinery of socialism, seeing no need to abandon it outright.

The third major development in Vietnam, too late for inclusion in this work, is the fascinating political science experiment now going on within the SRV National Assembly, converting it from a motivating and mobilizational tool of the Fatherland Front mass organization into a law-making, law-implementing body.

From the beginning, in the Democratic Republic of Vietnam, governance was by the Party. Policy instruction went downward from the center to the lower-level Party jurisdictions and to state enterprises. This instruction (Politburo orders, Central Committee directives, Party Congress resolutions, etc.) was written in ideological language. It worked fairly well during wartime, but with the complexities of economic decision making in the postwar years, it broke down. At the implementing level, orders were subjected to great and honest variations in interpretation. As with the Holy Bible, they could easily mean whatever one wanted them to mean. Advisors (not least those from the international funding agencies) counseled the Vietnamese leadership that one cannot direct a semimodern society in this manner. The Seventh Congress decided to hold a political science experiment. Government by Party fiat would be replaced by government by law. Policy would be transmitted by the Party leadership to the National Assembly, still in ideological language; and the Assembly would translate the instructions into laws to be sent down the line for implementation. This is the process now underway. Most significant of the 30-some pieces of legislation approved to date is the Law of the Land, which, while fuzzy in defining ownership (yours "for a period of time" to do with as you wish) drives a stake through the heart of the most cherished concept of Marx—collective ownership of the means of production.

Finally, while not wishing to make this an ad hominem review, I feel obliged to set forth briefly the author's record. Gareth Porter's reputation in the field has long been as a point man for the antiestablishment. He was the scourge of academics who supported the Vietnam War. Typical of his effort is "How Scholars Lie" (*Worldview,* December 1973). With George Hildebrandt, he defended the Pol Pot regime in Cambodia *Cambodia: Starvation and Revolution,* 1976). He argued at a congressional hearing that there was no bloodbath going on there (long after French intellectuals had done a *mea culpa* on the Khmer Rouge). At academic conferences he was often vindictive, occasionally downright mean (throwing the charge "war criminal" at those present who disagreed with him). Such was the passion of the times, and it was, if not excusable, at least understandable.

However—and this is the point—one finds none of this side of Gary Porter in the work here, not even vestiges. On balance, I would pronounce this an estimable book and commend it to all interested in examining Vietnam in depth. Porter has put to use his years of work on the subject and, in doing so, appears to have outgrown his earlier ideological orientation. We are all the better for it.

University of California, Berkeley DOUGLAS PIKE

The Politics of Economic Stagnation in the Soviet Union: The Role of Local Party Organs in Economic Management. By Peter Rutland. New York: Cambridge University Press, 1992. 297p. $64.95.

Peter Rutland asks how the Soviet political system worked between 1965 and 1990 and "why it was so resistant to reform" (p. 3). He approaches this through a detailed study of Soviet administrative behavior, focusing on the role of regional, local, and enterprise (PPO) party organizations in economic management. In updating earlier studies of party–management relations, Rutland makes some revisions but largely confirms the findings of Berliner, Azrael, and Hough.

The author introduces his study with a defense of the totalitarian model, then extends Friedrich and Brzezinski's "totalitarian syndrome" to describe the "post-totalitarian regime." Rutland emphasizes its ideological foundations—universal agreement by those who were politically active in the official system to preserve (1) elite rule, (2) the separation of public and private spheres, (3) monopolistic control over information flows, (4) the command economy, and (5) the Soviet Union's status as a superpower. (In addition, Rutland writes, they shared a view of politics in which the major differences within the elite were defined not by ethnic or regional interests but by generations.) In the conclusion, Rutland argues that a "crucial intermediate causal factor" in the collapse of the Soviet Union was the disintegration of this value consensus.

In the empirical chapters, Rutland examines Communist party intervention in the management of both industry and agriculture. He devotes special attention to the Party's role in regional economic coordination through its "prefects" (*obkom* secretaries), its role as "fireman" mobilizing resources for high-priority projects, and its control of personnel through its cadres policy. The evidence is drawn from a careful reading of Soviet-era publications, with particular emphasis on *Ekonomicheskaia gazeta.* He uses his empirical cases to substantiate the claim that Western analysts (using the "pyramid metaphor," p. 19) had overestimated the degree of central control over the lower reaches of administration; that Party and economic bureaucrats were guided by discrete political and economic logics; and that under Brezhnev, "party interventions in the economy were increasingly clumsy and ineffective" (p. 23). Rutland brings this empirical analysis up to the end of the Soviet system with a very brief discussion of administrative behavior after 1985.

In focusing his empirical analysis on the lower reaches of the Soviet bureaucratic hierarchy, Rutland may make it impossible to answer the most important questions he poses in his study. The central Party leadership could make heads roll, redirect investment funds on a massive scale, and thoroughly redesign bureaucratic structures. Instead, as Rutland's case studies show, the Brezhnev leadership usually chose to leave personnel in place, change investment only incrementally, and let the system grind away in its established routines. Why did the top leadership not choose differently? This question gets at the ultimate sources of economic stagnation and failure, but this study begs the question.

Because Rutland excludes the upper reaches of the Soviet system from his analysis, he omits the context that might give very different meaning to the administrative behaviors he observes. For example, the central leadership may have deliberately fostered the tension that Rutland notes between local Party and managerial officials. Rather than evidence of its ineffectiveness, this tension may have been a mechanism of central Party control. Again, the ministerial hierarchies may themselves have been mechanisms of central Party control. Thus, the victory of ministerial hierarchies over local planning and coordinating may have represented the victory of center over periphery and a victory of the Party. Again, the Soviet press's spotlight on deviations from the expectations of the central Party leadership (Rutland's primary evidence) may be not a sign of failings of Party control but actually a means of control. This study does not give us a picture of the routine workings of the economy and the enormous power of "anticipated reactions" to contain the behavior of the economic managers. In short, without the big picture, it is difficult to impute meaning to the smaller details in the study.

Following on his detailed study of administrative behavior, Rutland's conclusions are somewhat baffling. First, he concludes that "the economic stagnation which crept over the USSR during the Brezhnev era was in no small part due to the close monitoring of economic life exercised by the CPSU" (p. 218). Yet the author seems to make the diametrically opposite case throughout the preceding chapters—that in the Brezhnev era, stagnation resulted from the growing inability (or unwillingness) of the CPSU to move economic administrators from their established routines. Second, to find the primary cause for the collapse of the Soviet Union, Rutland reaches completely outside the bounds of his study. The crucial factor, in his estimation, was "the multinational character of the state" (p. 220). (Once again, the research design seems inappropriate to the questions with which the study begins.) Third, his "intermediate" cause, which is closer to the subject of the monograph, seems to shift his focus from the micro behaviors on the shop floor to the macro phenomena of the Soviet political system—the breakdown of shared values in the political elite. In short, while Rutland has written a fine study of administrative behavior *under* the *ancien regime*, it is not entirely clear that he has explained how the regime worked, why it stagnated, or why it collapsed.

Despite the apparent mismatch between its larger conceptual framework and its empirical analysis, Sovietologists and post-Sovietologists should find much of value in this monograph. Rutland has probably written one of the final Sovietological studies of party–management relations, synthesizing the major findings from the existing literature. Through close reading of the Soviet press, he advances this analysis up to 1985. Rutland has established a solid base on which future historians and social scientists can build with new information culled from interviews and archives.

University of California, San Diego PHILIP G. ROEDER

The Rise of Meso Government in Europe. Edited by L.J. Sharpe. Newbury Park: Sage, 1993. 327p. $65.00.

This book, originally conceived at a European Consortium of Political Research conference held in 1986, is the product of a series of meetings held in a number of desirable European locations. It is concerned with the phenomenon of the *meso* (taken from the Greek word mesos, meaning middle). The central thesis is that the unitary state in western Europe has experienced developments over the last 20 years or so which have fundamentally changed its character. Following a general survey by the editor which both helps to link the individual contributions as well as providing a useful overview of the literature, the volume comprises eight country studies—Belgium, France, Italy, the Netherlands Norway, Poland, Spain, the United Kingdom—together with a brief note on Denmark and Sweden. In addition, there is a chapter on the place of regions within the European Union. The style varies. Some of the contributions—such as that on Poland—offer an account of governmental structures with little analysis of the concept of the meso. Others, such as Toonen on the Netherlands and Keating on regions and the EU, help the reader to reflect on the general concept and its implications for the political processes.

What then is the meso? Trying to provide an answer to this question on the basis of the papers in the book is reminiscent of the "blind man and the elephant" phenomenon in the sense that definition depends on context. The editor states on the first page of the introduction that the meso should not be equated with the region. Rather, he argues, it is not so much a level of government as a "decision-space" within the governmental structures where certain kinds of decision are taken. Nevertheless, even he lapses into the usage "level of government." The contributors appear to use the term to indicate various phenomena, most often the region or some structure approximating to a level of authority between central and local government. When this is combined with the predictable variations characteristic of national experiences together with uncertainty as to whether the meso exists at all in some cases (Belgium, discussed in Delmartino's chapter, for example) some inevitable uncertainty enters the reader's mind.

The causes of the rise of the meso are set out in a clear fashion in the editor's survey and encompass the kinds of development which one might expect. Thus regional ethnic nationalism, functional adaptation to changing responsibilities, particularly those associated with the growth of the welfare state, and regional planning accompany ideology, political interests and "me-too-ism" among the explanatory factors. The situation, of course, varies from country to country. In the case of France, for example, Mezey argues that the key factor has been the response to the needs of modernization.

A perspective which is relatively underplayed is that of domestic responses to the forces of globalisation. Keating's contribution on regions in the context of the EU is helpful here, as is that of Toonen on the Netherlands. Keating argues that we are witnessing the emergence of a new political dynamic focusing on a triangular relationship between Europe, the nation state and the region which gives rise to complex patterns of interaction. Toonen extends this outside the European context by pointing to the links that bind regions and cities (such as Rotterdam) to the wider international environment.

This takes us back to definitions: the *middle* of what? The problem is that the term *meso*, despite Sharpe's initial disclaimer, implies a position within a hierarchy of political activity from the subnational to national to international. And yet much of what is described here is part of a broader and even more complex redefinition of political processes wherein actors are frequently constrained to operate outside their allotted arenas. In this context, it is not at all surprising that one of the recurring themes in the book is the relationship between non-central authorities and the EU. Clearly in the Spanish situation as described by Cuchillo, access to Brussels has been a bone of contention between the regions and Madrid and has yet to be resolved.

But as Keating suggests, the position of regions on this issue is complicated by two sometimes conflicting needs: a degree of local autonomy to pursue interests on the one hand and access to centers of decision-making which affect those interests on the other. Increasingly, those centers lie outside the domestic arena. The consequence is ever more complex patterns of linkage, not simply between center and region/locality but between a variety of actors, governmental and non-governmental, both within and without national boundaries. In other words, Sharpe's "decision space" (p. 1) is becoming internationalised. Internal interdependencies between levels of government, a well-recognized result of adaptation of the polity to the needs of policy management, are being joined to international interdependencies reinforced by the globalization of capital but also by a range of transnational linkages which focus on the region as much as, or more, than its national setting. Consequently, the meso is continually shifting—or, as Toonen describes in the Dutch case, there are several mesos, sometimes in conflict with each other.

As both Sharpe and some of his contributors make clear, the pressures with which this book is concerned do not presage the demise of the nation-state. Rather, we are witnessing an evolutionary process whereby policy functions are discharged at a variety of levels. National governments may come to perform fewer functions in some contexts, more in others. Whether or not we are witnessing the construction of a "Europe of the regions" (if this implies the disappearance of the nation-state it seems unlikely), the policy arena will become more complex and the *meso* will have a place within it.

But increasingly the question will be: what is it the *middle* of?

Coventry University BRIAN HOCKING

The Rising Tide of Cultural Pluralism: The Nation-State at Bay? Edited by Crawford Young. Madison: University of Wisconsin Press, 1993. 305p. $49.50 cloth, $17.95 paper.

Until recently, political scientists have been loath to treat the dynamics of ethnic politics seriously. They assumed that modernization required a strong, secular state and that ethnic attachments represented annoying impediments in the way of cultural assimilation. The more rapidly these hindrances to progress could be swept aside, they concluded, the sooner state elites would be in a position to move their societies toward nation-building and modernization. This was a case of misunderstanding and misperception writ large. Because the ethnic community, whether real or mythical, has played an important role as a demand-bearing group of a psychological nurturer, it has survived and developed, for some purposes at least, as an alternative to the state itself. It is therefore time for political scientists to revise their frames of analysis and to take account of the complex and fluid interplay of state-societal and interethnic relations now unfolding rapidly.

How are we to interpret and understand the radically changing process of identity politics in post-Cold War times? Crawford Young's newly edited book, *The Rising Tide of Cultural Pluralism,* provides an initial boost to analyzing the multi-faceted forces at work. In his introductory essay, Young develops a broad theoretical framework for studying cultural pluralism, setting forth the three formative conceptions of instrumentalism, primordialism and constructivism. Whereas instrumentalists emphasize the role of communal broker in mobilizing his or her ethnic constituents to extract material resources from the state, the primordialists perceive group identities largely in terms of their powerful emotional and psychological holds on their memberships. The constructivist label is applied to those scholars who view the ethnic group as a creation of political and social elites and who take advantage of a momentary historical opportunity to establish a new (albeit artificial) grouping. Young's modes of conceptualization are not new. Yet he does advance the current dialogue by stressing the overlaps and points of convergence between the instrumentalist and primordialist views. "Primordialism," he writes, "usefully completes instrumentalism by explaining the force of the 'affective tie' through which interest is instrumentally pursued" (p. 23). Primordialism and instrumentalism, then, are dimensions of a larger ethnic phenomenon. If social scientists accept this synthesis, we will be well on the way to going beyond definitions to the analysis of process and policy.

A number (but by no means all) of the contributors to this volume apply Young's conceptualization in the chapters that follow. Growing out of a summer seminar for college teachers, the result is an uneven coverage of conflict themes and situations—with the former Soviet Union, the United States, and Ethiopia probed most extensively. In an interesting essay on language policy in the United States, Ronald J. Schmidt interprets language politics within an instrumentalist framework, contending that "competitive ethnic relations will devolve into a competition over who will bear the greatest 'costs' and which group will garner the most 'benefits' in the mutual adjustment to multi-lingualism in the political territory which the groups share" (p. 82). Douglas Spitz, focusing upon cultural pluralism in contemporary India, writes convincingly about the Rashtriya Swayamsevak Sangh's use of religious and cultural symbols to promote feelings of communal kinship and resistance to multi-nationalism, separatism, and secularism. Finally a number of authors—Nader Entessar ("Azeri identity in the Azerbaijan Republic and Iran"), Tekle M. Woldemikael ("Eritrean social pluralism"), James Quirin ("The Beta Israel of Ethiopia"), and Alan LeBaron ("Maya ethnonationalism in Guatemala")—adopt the constructivist perspective and describe their respective identity groups as "imagined communities."

The commonalities and differences among these perspectives are not carefully probed, leaving the task of comparative explication largely to the reader. What measures are we to employ to determine whether such identity groups as Bosnian Muslims or Serbs, Tamils, or Hutu are formed to further instrumentalist, primordial, or constructivist purposes? When does the imagined community cross the threshold to a genuine sociopolitical community? If peoples mobilize along communal lines and are prepared to sacrifice their lives to advance identity group objectives, does the constructivist framework adopted by many of these contributors help in interpreting the dynamics of competition and conflict?

With their large brush strokes, Young and his colleagues point us toward the need for further comparative analysis. Once this is done, we will be freed up to concentrate on other problems—most notably, systematic attention to the interactional process at work, the relationship of economic and political liberalization to cultural pluralism, the impact of the international variable on many tense ethnic conflict situations, and the effective management of identity group conflicts by state elites. Young is right that "[t]he postwar flow of political history has . . . radically transformed the politics of cultural pluralism" (p. 21). This leaves us with much to do.

University of California, Davis DONALD ROTHCHILD

INTERNATIONAL RELATIONS

The Morning After: Sexual Politics at the End of the Cold War. By Cynthia Enloe. Berkeley: University of California Press, 1993. 326p. $38.00 cloth, $15.00 paper.

Cynthia Enloe, one of feminism's liveliest minds, is at it again. In *Bananas, Beaches, and Bases: Making Feminist Sense of International Relations,* she dared to discuss the Chiquita Bananas of international political economy, the Pocahontases of diplomatic practice, the female companions of realist warriors on military bases, and the chambermaids western tourists failed to notice when going international for an "exotic" vacation. All of these people, she contended, were in international relations and should be accounted for by the field of IR. Indeed, it took considerable power to create and then exile these people and activities as irrelevancies next to the global management of interstate relations. In *The Morning After,* Enloe extends the themes of her earlier work to the supposedly new era we have entered at the end of the Cold War. Her overarching point is that various forms of masculinity and femininity were required to create the Cold War, and that various sorts of transformations in the relationships between women and men are needed to ensure that the ending processes, which are multiple and ongoing, proceed.

Much of her discussion revolves around the concept of militarization, a word she defines minimally as any part of a society that becomes controlled by or dependent on the military or military values. There is much around us that can be militarized, such as toys, marriage, scientific research, university curricula, motherhood, fatherhood, AIDS, immigration, or comic strips. The Cold War, she argues, depended on multilocated and thoroughly permeating militarizations of identity and security. Men had to be instilled with a type of masculine identity that made it possible to commit thousands of mostly male bodies to places like Korea and Vietnam in the name of securing a distant national interest. Women had to play their parts as domestically-based yet vulnerable patriots, armed with a sense of what real, what proper, what brave men did for their countries (armed later with yellow ribbons of support for troops abroad). When the "little" wars of the Cold War finally wound down, thousands of men had to be deprogrammed so that masculinity could take on peacetime dimensions. Thus, from the ashes of war fathers were born; of course, the burdens of militarization merely shifted to men holding the line for us in foreign armies, men trained in our ways of Cold Warring. Some women had to relinquish elements of independence and autonomy gained while their men were away: they mothered, soothed, or turned from pristine patriots into Fatal Attractions. Others were encouraged to service the outposts of freedom by offering love for money.

Enloe argues that the process of militarizing masculinity and femininity, however, was never easy and straightforward. Just as it took power to create Chiquita Banana as a marketing logo for the Banana Republics that political economy had also created, it took extraordinary power to create and sustain militarized gender during the Cold War. Says Enloe, "if masculinity 'in the raw' were sufficient, there would be little need for the sweat, blisters, and humiliations of basic training" (p. 55). If femininity were a cup of sweet natural nectar to be preserved from alien ideologies, why was militarization in Central America and the Caribbean accompanied by efforts to bring women into the low-paid, underprotected, decidedly unsweet sweatshops of foreign companies?

Enloe's gaze takes us on the type of global journey that rarely figures into mainstream IR. We consider Nicaraguan voters electing a woman to office, Soviet women engaged in anti-war protests, Filipina prostitutes, Canadian dissenters from NAFTA, American servicewomen raped by their fellow soldiers in the Persian Gulf War, Kuwaiti feminists, NATO politics, and UN peacekeepers. In each case, what makes the story noteworthy is that it illustrates some aspect of the militarizing reach of the Cold War or some fragment of a post-Cold War world that continues to be defined by Cold War values (when will Americans turn out for a parade to mark the return of UN peacekeepers? Why do some of the strongest manifestations of militarized Zulu identity coincide with the post-Cold War leap toward postapartheid South Africa?).

This book, sturdily buttressed by Enloe's previous work on feminist international relations, is exemplary in the refusal to refuse the lives of everyday people in order to do IR. It is exemplary in concerning itself with everydayness everywhere and not just with "the public" in the great powers we normally study in this field. It lays down important ground when it suggests that IR, the home of power politics, actually underestimates power by failing to consider how much power it takes to militarize gender and how much of our world politics depend on this militarization. The point: relations of sex and gender are not incidental to world politics. They are not topics some of us can simply choose to research because we are feminists. Rather, these relations illuminate IR's core concerns with war, peace, and security and should become part and parcel of the field.

There are ways, however, in which this book is somewhat marred. Enloe does not make a fully persuasive case that many of the militarization strategies she discusses are especially indicative of Cold War fighting as opposed to other militarized conflicts, other wars. She does not problematize or periodize the Cold War, nor question the gender identities of the men and women she interviews and writes about. "The Cold War" might be a feature of all patriarchal peaces rather than the mark of a period we have been trained to recognize in a certain way. "Men and women" may not see themselves as men and women acting thus and so during "the Cold War." Sometimes, they may see themselves enmeshed in a plethora of relationships (e.g., as soldiers, mothers, workers, party members) that are not easily summarized as militarized gender relations, in which case, whose morning after is Enloe studying? When I used the book in an advanced undergraduate course on women, power, and politics, moreover, I also found that the students were not certain they understood what militarization was and whether it was something different than patriarchy. A little more time with definitional issues might have strengthened an already powerful set of arguments.

Finally, there is the issue of theory. Enloe has been accused in the past of being atheoretical, in that she does not present the reader with a discipline-recognizable framework for her arguments, nor endeavor to refer her findings back to IR theory in a way that could compel the field to review its inadequacies. She prefers action stories to theory. She prefers writing for the lay person and the feminist and the bloke with common sense to writing for the imprimatured experts of IR. Arguably, her approach simultaneously challenges IR's preoccupations and scuttles away from the inevitable confrontation it invites with the field.

Yet to quibble with her way of showing how everyday people and the dailiness of their lives comprise aspects of international relations is to privilege the usual way we in the field understand theory. To ask for more definitions and tracings of how the many examples she offers illustrate militarization is to take from the reader the distinct pleasure and challenge of reading IR differently, more evocatively. To ask for a querying of men and women as people we may not be able to identify and label easily is to risk making postmodernist sour grapes in the face of a champagne achievement of standpoint feminist IR. This book effervesces. It will tingle stodgy IR palates and be consumed in one bubbly gulp by the many merry-makers about whose tastes run to spicy stews.

Australian National University CHRISTINE SYLVESTER

Double-Edged Diplomacy: International Bargaining and Domestic Politics. Edited by Peter B. Evans, Harold K. Jacobson, and Robert D. Putnam. Berkeley: University of California Press, 1993. 490p. $50.00 cloth, $15.00 paper.

This book is an important landmark in what has become a virtual flood tide of recent works deemphasizing the international system as an explanatory focus. In contrast to others that situate themselves theoretically smack in the middle of the domestic arena, this treatment is located precisely at the boundary of the international system and domestic politics. As such, it recognizes both systemic roots of foreign policy decision making as well as domestic influences on the calculus of such decision making. However, the nomenclature is not the familiar one of "balance" or "disequilibrium," but a new one introduced largely by Robert Putnam's well-known article entitled "Diplomacy and Domestic Politics" (*International Organization*, 1988). Indeed, the entire book emerged from several conferences held at the Center for Advanced Study in the Behavioral Sciences, dedicated to exploring Putnam's formulation in a series of case studies using the method of focused comparison. Concepts such as "win-set"—the set of international agreements that would gain domestic approval—or "reverberation" wherein international pressures "reverberate" within domestic politics, constitute the new lexicon.

The book is divided into three principal sections, viz. security issues, economic disputes, and North-South tensions. As one might expect, this formulation works least well in matters concerning national security, where international threats can temporarily mute the contentiousness of domestic interests. Jack Snyder clearly shows how domestic changes that initially appear to be sui generis actually are consequences of international changes that yield learning by decision makers of the objective constraints and incentives of the system. Understanding bargaining behavior of East and West over Germany entails not only some of the diplomacy occurring at the boundary between system and state suggested by Putnam, but also as Snyder suggests, a knowledge of the separate processes in both system and domestic society that occur independently and then can have separate impacts on the negotiation process.

At times, the system is slighted in favor of domestic processes as in Janice Stein's treatment of the Camp David accords. Although the Egyptian economy and Israeli domestic politics clearly affected the Camp David process, nevertheless, it was the 1973 Yom Kippur War and especially Henry Kissinger's encouragement of a military disengagement, that introduced elements of a power balance missing from the Middle East since the 1967 war. This new balance facilitated the making of the later accords. Although mentioned in her analysis, this process is treated as a "constant" instead of the variable initially manipulated by Kissinger to lay the groundwork for a later peace, and of course, increase United States influence in that region.

In his introduction to the volume, Andrew Moravcsik also tends to avoid systemic explanations in favor of domestic ones. Brezhnev is accused of inconsistency in his policies of accommodation in Europe and belligerency in the Third World; such inconsistency is explained with reference to the need to placate domestic hardliners. However one need not adduce ad hoc explanations. In my own work, for example, the system itself after the Hungarian, Berlin, and Czechoslovakian crises gave evidence of the extreme danger of further confrontations in Europe, while displacement of superpower conflict to the Third World actually helped foster system stability. The demands of parsimony and simple structure of explanation still must be met, even as we open new theoretical venues.

Two of the most interesting and informative chapters are those that deal with violated expectations as to probable outcomes. Given the realist paradigm, the more powerful target of influence should be better able to resist the blandishments of a great power. Yet, in two instances, quite the opposite occurred. The first is by John Odell, who describes the United States failure to prevent Brazil from developing its national computer industry and in doing so displacing U.S. and other foreign firms, and contrasts it with U.S. success in removing new barriers to U.S. feed grain exports to Spain and Portugal as new members of the European Community. Success in the EC case but failure in Brazil occurred despite sizable power differences between the EC and Brazil, and the far greater economic dependency of Brazil on the U.S. The answer turns on new theoretical contributions by Odell that emphasize the role of domestic interests in making threats credible or undermining their credibility, as the case may be.

The second and perhaps the most enlightening of these contributions is by Lisa Martin and Kathryn Sikkink, who make human rights issues not only morally compelling but theoretically important. Why did the rather large and important South American power, Argentina, succumb to U.S. pressures concerning human rights abuses while the much smaller and relatively insignificant Guatemala was able to resist such pressures? Again, the answer lies in the domestic arena in which the absence of domestic human rights organizations in Guatemala and in particular, the view of the rural countryside as the preserve of the military that would countenance no peasant opposition, doomed the Carter administration's efforts to be seen as interventionist and hence to be rejected. In contrast, the presence of such organizations in Argentina ultimately facilitated the U.S. effort. There are other excellent treatments in this book, but space limitations prohibit an extended discussion.

Overall, the most theoretically significant contribution is the concluding chapter by the Berkeley sociologist, Peter Evans. He is able to generalize across the several case study comparisons and transcend their immediate theoretical and empirical domains. Evans relates the probability of agreement in negotiation to the extent to which costs are concentrated and benefits are diffuse. The extent of enfranchisement appears to bear no relationship to the probability of agreement. This latter conclusion, drawn in part from the Guatemalan instance, appears to defy conventional wisdom on the likelihood of agreement being dependent on the number of voters and voting groups in society. Here, we find one of the strong contributions of this book. The overall effort is not one that simply confirms the obvious in stressing the importance of domestic interests in order to reach international agreement, but one that yields some counterintuitive findings that can be built upon in the future.

Theoretically, where does this book fit in the overall scheme of things? Clearly, at this stage it is no more than a theoretical framework for analyzing the success and failure of international negotiations. This is not to diminish its importance, for international negotiations can be exceedingly important, but simply to suggest that it is not pitched at the level of grand theory. With the publication of this book, Putnam's initial contribution is now more than a metaphor of analysis, as he himself suggested it might be, and far more than a loose framework for the incorporation of a chronology of events. This is the potential degenerate instance of an application of Putnam's framework that this volume happily avoids.

Yet in one respect, this book constitutes a retreat from the level of sophistication of Putnam's initial formulation. In that article (included as an appendix), there are at least the beginnings of formalisms that could have been built upon. None of these are in evidence in this edited collection that rests at the level of verbal theory. Inherently, there is nothing wrong with this, but it does leave the reader less satisfied than if at least some of the contributions had dealt with the formal aspect. Also missing, as Evans indicates in his concluding chapter, is a sense of the historical. These studies are almost entirely cross-sectional in design. A larger sense of change over longer time periods would be a valuable addition to this now successful research program.

What is now to be done? Increased formalization and an incorporation of the longitudinal are clear possibilities. Equally important might be a large N quantitative analysis of the validity of several of the propositions gleaned from these case studies. What is clear is that this essentially brush-clearing operation has been successful and future researchers are now in a position to build on these efforts.

Rutgers University MANUS I. MIDLARSKY

The Best Defense: Policy Alternatives for U.S. Nuclear Security from the 1950s to the 1990s. By David Goldfischer. Ithaca: Cornell University Press, 1993. 283p. $42.95.

For much of the nuclear age, Americans and Europeans have learned to live with the paradoxical notion that security rested on a balance of mutual annihilation. Most of the time, however, most people disliked this situation. It was either morally unsettling or seemed to require unwarranted trust of our adversaries, or both. These circumstances appeared to be caused by a combination of technological necessity (given the massive destructive power of nuclear weapons) and political rivalry. Neither element easily yielded to change.

The end of the Cold War removed much of the political compulsion to maintain the course of security by mutually assured destruction (MAD). David Goldfischer suggests, furthermore, that MAD cannot be adequately understood as a function of the technological imperative. Goldfischer argues that a middle ground for nuclear security, between the pursuit of war-fighting and submission to assured destruction, has existed since the beginning of the Cold War. *Mutual defense emphasis* (MDE) arms control was promoted, sometimes within the U.S. government, in the early 1950s, the early 1960s, and again during the Reagan years. On each occasion, the combination of mutual offensive restraints and strategic defenses lost the bureaucratic battles in favor of exploiting U.S. strategic offensive advantages. The resulting MAD world of offense-based deterrence and arms control that prohibited strategic defenses was wholly predictable from the politics of the process.

The process is well revealed in Goldfischer's chapter on early proposals to combine restraints on hydrogen bomb tests with pursuit of continental defenses. J. Robert Oppenheimer and others who pressed for this policy direction came under suspicion of disloyalty, and the proposals were tarred by the same brush. In addition, Goldfischer characterizes the strategic inclinations among the warring parties in the Defense Department (for whom much more was at stake than for the arms-control outsiders) as an emerging "cult of the offensive" that narrowed the range of acceptable security policies.

Mutual defense emphasis must be paired with offensive arms control to be a viable nuclear security policy. Neither unrestrained defense nor mutual offensive reduction alone is prudent, according to Goldfischer. The first aggravates insecurity, insofar as unilateral pursuit of defenses may create ambiguity about the defender's intentions, especially when some defensive technologies and deployments, such as strategic defense initiative, are capable of offensive applications. Deep mutual reductions, on the other hand, create unnecessary vulnerability, because offensive deterrence becomes unstable at very low numbers of weapons.

Goldfischer does not argue for anything like the Star Wars program of the 1980s, despite his defense of defenses. In fact, he offers a very sensible set of criteria for MDE arms control that explicitly excludes space-based deployment. The first two elements in this list, for example, are "a pledge not to deploy defenses unilaterally beyond the confines of the 1972 ABM Treaty" and "a proposed ban on space-based weapons of all types, including restrictions on development enforced by means of a ban on testing in space" (p. 248). These prescriptions, along with continuing the offensive reductions undertaken in the INF, CFE, and START treaties, place Goldfischer's argument squarely within the bounds of mainstream arms control. It is his case for undertaking mutual defenses *in conjunction with* offensive restraint that challenges the conventional wisdom.

This otherwise fine book suffers, however, from a tone and language that reveal it as a victim of its times, most obviously in its first and last chapters. Goldfischer acknowledges the end of the Cold War and the demise of the Soviet Union; but this is fundamentally a book about the U.S.–Soviet rivalry, and he seems unsure in talking about the post-Soviet set of relationships. Admittedly, 1992 (the last year in which the manuscript was likely to be changed or updated) was also the first year of the post-Soviet order, and foretelling what the future held was particularly difficult. Still, simply discussing the "nuclear superpowers" as if nothing had changed except the leadership of one side is not entirely satisfactory.

Russia and many of the former Soviet satellites seeking to join NATO or the more general Partnership for Peace and Washington and Moscow's retargeting their strategic forces to the North Pole, rather than at each other, raise several questions. Is the pursuit of mutual defense emphasis necessary in this new world? Arguably, so long as some states possess long-range nuclear weapons systems, a defense against accidental or very modest launches may be prudent. But it must be evaluated in its relevant political context and in light of alternatives. Ongoing debates about tactical ballistic missile defenses are related to these problems. Similarly, MDE may be a key to expanding the regime of offensive reductions from the United States and Russia to include Britain, France, and China. But at least three—and perhaps four—of these players do not have a problem with nuclear threats against each other, so the nature of the threat is less clear. Finally, is MDE poltically possible? In an era when defense spending is increasingly yielding to domestic priorities, the necessity of a new, extensive strategic defense effort will be a tough sell.

Harvard University WILLIAM B. VOGELE

Ideas and Foreign Policy: Beliefs, Institutions, and Political Change. Edited by Judith Goldstein and Robert O. Keohane. Ithaca: Cornell University Press, 1993. 308p. $37.50 cloth, $14.95 paper.

Idealism has been sneaking up on international relations theory for a couple of decades now. Though few students of world politics today call themselves idealists, many are focusing their research on the ideational and social causes of state action, rather than the material ones favored by realists: political psychologists on belief systems and operational codes, liberals on international institutions and democratic values, critical theorists on ideology and discourse, and even "cultural realists" on nationalism and religion. Despite the crowded field, Judith Goldstein and Robert Keohane have assembled an impressive set of new essays that goes a long way toward giving ideas analytical respectability as causes of state action, even if they also miss some opportunities to clarify and push the thesis further.

Ideas and Foreign Policy opens with a strong theory chapter by Goldstein and Keohane, in which they propose a common analytical framework hammered out in three meetings with the other authors. Positioning the project against rationalists for favoring interests over beliefs, critical theorists for failing to develop a research programme, and cognitivists for focusing on belief accuracy rather than content, they argue we need to show not only *that* ideas matter but *how*. On this basis Goldstein and Keohane define as the null hypothesis for the volume that variation in foreign policy can be explained by factors other than ideas, principally power and interest (an analogous null governs much of regime theory). They then identify three types of beliefs (worldviews, normative, and causal), and three "causal pathways" by which they may affect foreign policy: *road maps* to reduce uncertainty, *focal points* to solve coordination problems, and *institutionalization*. The theory section is rounded out with a superb chapter by John Hall on the materialist-idealist debate in classical sociology.

There are eight more empirical essays organized around the causal pathways. Exemplifying road maps, John Ikenberry shows how Keynesian economic ideas shaped American and British national interests in postwar trade negotiations; Nina Halpern analyzes the role of Stalinism in helping states do socialism; Robert Jackson focuses on the role of normative ideas in decolonization, as does Kathyrn Sikkink regarding human rights policies. Geoffrey Garrett and Barry Weingast integrate ideas as focal points into a rationalist model of the Single European Act, and John Ferejohn examines the effects of competing ideas of monarchy in 17th century England. Stephen Krasner makes a materialist argument that ideas did *not* matter in the Peace of Westphalia, and Peter Katzenstein details the effects of institutionalized norms in Japanese and German postwar internal security policy. These chapters are methodologically sophisticated, substantively interesting and, Krasner's excepted, provide compelling evidence for the independent causal force of ideas.

The volume differs from other idealisms (or "idea-isms") in two important ways. The first is its broadly rationalist cast, which separates it from the otherwise similar (yet strangely neglected) literature on belief systems. Although Goldstein and Keohane criticize rationalism for focusing only on preferences, as I understand it the core of rationalist explanation is that desire or preference *plus* belief or expectation causes action. Game theorists, after all, now tell us that "common knowledge" helps solve games with multiple equilibria. As such, most of the chapters do not challenge rationalism so much as rejuvenate its ideational component. A second contribution is the attention to micro-foundations. The editors do not emphasize this, but the ideas discussed are those held by individuals, not corporate actors or social systems. This enables the authors to provide detailed descriptions of the mechanisms by which ideas cause foreign policies, which are often lacking in more holistic or institutional idealisms, including regime theory.

The volume also misses two important opportunities. One stems from an ambiguity in the dependent variable. Goldstein and Keohane and several other contributors accept the point—long pressed by critical theorists—that ideas may shape interests as well as behavior; one effect operates on a property of actors themselves, the other merely on what they do. Yet, there seems to be little recognition of how different these effects really are, and of its significance for larger theoretical debates. If ideas only shape behavior, materialists can say they are at most "relatively autonomous" of material forces that still determine interest "in the last instance," and a dualistic model of man in which preferences and expectations are rigidly separated is not threatened. If ideas sometimes shape interests, however, as Ikenberry and Jackson show, then both the impact of power and the nature of interest will depend on beliefs. Unfortunately the volume's research design is not set up to distinguish and compare the two effects. Indeed, despite the concern to show *how* ideas matter, the book's null hypothesis in fact permits us to infer only *that* they do. To answer *how*, a second null that ideas only affect behavior might have been set up, and then analysis directed to whether interests really are a function of ideas.

The other issue concerns the relationship between the ideas held by individuals and the institutional structures in which they are embedded, notably the state and international system, which are themselves constituted by ideas. Several chapters invoke this relationship, but none addresses the question of whether institutional structures are reducible to individuals' ideas ("common knowledge"), or have a facticity independent or constitutive of individuals (what is today sometimes called "supervenience"). The rationalist sub-text of the book suggests that most of the authors would favor the individualist, common knowledge view over the more holistic, supervenience approach. If institutions are reducible to common knowledge then we should study ideas much as these authors have, starting with individuals and aggregating to capture social and institutional effects; if not, there will be an important aspect of world politics that can be grasped only by examining *collective* (as opposed to shared or common—the distinction is Durkheim's) representations like discourses and ideologies. The latter are the primary focus of more explicitly critical or "structural" idealisms, as well as of Jackson's paper, to this reviewer's taste the best of the lot. The book leaves us with a significant challenge of balancing the micro-foundations of individual beliefs with a conceptualization of corporate agents and international institutions as emergent collective knowledge structures.

Ideas and Foreign Policy is full of interesting lines of argument that bear further development, and even in its problems raises important issues that will advance the idealist research programme. It is a must-read for idealists and their critics alike.

Yale University ALEXANDER WENDT

Foreign Relations and Federal States. Edited by Brian Hocking. New York: St. Martin's, 1993. 294p. $57.00.

This volume examines an important, though very understudied, phenomenon in the field of international relations. The lack of attention to federalism as a source of foreign relations is unfortunate because politics in a substantial number of countries worldwide involves tensions between (and among) territorially based actors and central authorities. This is evident in the breakup of the former states of the Soviet Union, Yugoslavia, and Czechoslovakia as well as in ongoing political pressures and change in Canada, India, Germany, South Africa, and so on. Less obviously, federalism is an important dimension of routine domestic politics in a large number of Western states (e.g., Germany, Australia, the United States) and the numerous ethnically divided African and Asian states. It is, then, curious that these pervasive, fragmenting pressures on foreign relations have not received greater attention in the field of international relations, especially given its preoccupation with the territorially defined nation-state.

This thought provoking, well-organized collection of essays makes an important, though not comprehensive, step in that direction. The majority of the volume's 17 chapters are case studies examining closely a particular issue and/or country's system and foreign affairs. The countries included in the volume are the larger, major advanced industrial democracies with federal systems: Australia, Canada, Germany, and the United States. Case study chapters vary in theoretically meaningfully ways with respect to the scope and type of actors considered. Some are single (or dual) country studies, several look at federal systems in interaction (e.g., Australian states, Canadian provinces, and the Chinese central government), and others focus mainly on non-central types of actors, e.g., subnational actors (Ontario and Quebec) or supranational bodies (the European Union). In the most basic sense, the value of these case studies lies in their providing substantive, or descriptive, insights into the kinds of international activities created by federal arrangements and their distinctiveness from the traditional foreign policy behaviors of central governments.

But this is not to say that the book is atheoretical. The book's four opening chapters take different theoretical or conceptual cuts at approaching federalism as its linkage to international affairs. At one level, they focus on basic conceptual tasks required to capture the apparent cross-national complexity of, first, the variety of constitutional arrangements linking states and provinces to the international arenas and, second, the classification of different types of "non-central foreign economic behavior" and activities disputed by "central" and "non-central" activities. At the other level, the essays consider what factors drive the increasing involvement of states and provinces in international affairs. Particularly useful in this regard is John Kincaid's original essay in which he argues that growing international economic interdependence has created a tension between "citizenship" and "consumership" involving a greater demand for the relatively local, or non-central, regulation and coordination of international ties. These kinds of analytic insights are not limited to the first four theoretical chapters. Almost all case study authors have interesting theoretical and conceptual points about the form and sources of federal pressures in their particular situation. Though often made briefly, these deserve careful attention, often best by comparing related chapters. For example, the chapters on Quebec and Ontario by Louis Balthazar and David Dyment, respectively, are useful in conceptualizing the different situations of the two provinces and showing that much of dissenting Quebec's international activity falls short of dramatic separatism while the more federally content Ontario engages in considerable autonomy in its international contacts. The essays by Michael Smith and Christopher Hill are important steps toward understanding the European community as a single international actor. Taken together, then, the volume has a strong theoretical strand running through it. If there is one complaint, it is that there is no concluding chapter to tie together the various strands running through these chapters. Given their diversity and their originality, a closing synthesis (if yet possible) of different arguments and approaches—and their implications for further research—would have been valuable. Still, the book remains coherent given the solid introduction and logical flow of chapters.

Given its broad title, it is important to recognize that this book is not comprehensive and takes a particular theoretical approach. In fact, the book might have been better titled "*transnational* relations and federal states." Reflecting the work of Keohane and Nye (but with interesting non-U.S. perspectives), almost all of the book's chapters view federalism within the context of increasing "complex interdependence." In doing so they raise two intertwined considerations. One is that "non-central" political entities, such as states and provinces, need to be considered in disaggregating the state to uncover the "multiple channels" that now link national societies and propel transnational ties. These entities are different from private "transnational" actors such as multinational corporations; instead, like "transgovernmental" players, these actors operate authoritatively but presumably far more independently than bureaucracies of the central government. This leads to the other theoretical theme: that the "coordination" of the authoritative activities of "non-central" and "central" actors are likely to be yet another governing problem affecting national leaders coping with growing interdependence.

Some topics are not covered, which is quite legitimate, but because of the book's broad title the prospective reader should be aware of these. First, of course, the sample of nations is quite narrow. Authoritarian states, past and present, are not included; nor are the many ethnically fragmented states throughout the non-Western world. In fact, even such smaller advanced industrial democracies like Switzerland and Belgium are excluded. Second, the focus is clearly on the management of less controversial kinds of activities. Political crises surrounding ethnic conflicts and separatism are not considered; how central foreign policy makers deal with *these* severe "two level games" is not addressed. Third, the implications of federalism are not fully addressed with respect to the decision making within the central government. Little is said about the representation of regional interests in the legislatures and executives of central governments. The emphasis is solely on "non-central" actors as a somewhat autonomous channel in a transnational world. What is missing, and this is my sole criticism of the book, is how "non-central" influence is wielded within national institutions and how this, in turn, affects national foreign policy. The representation of sectoral interests in national legislatures and coalitions is an important second dimension of federal foreign relations, even among the stable advanced industrial democracies considered in this book. This, though, could be considered in a subsequent volume; the current volume has accomplished a great deal despite these gaps.

West Virginia University JOE D. HAGAN

Localizing Foreign Policy: Non-Central Governments and Multilayered Diplomacy. By Brian Hocking. New York: St. Martin's, 1993. 249p. $59.95.

The activities, influence and concerns of local governmental units—states, provinces, regions—in the formulation and implementation of the foreign policy of their respective states appears to be a matter of increasing interest. Local governmental concerns with issues such as environmental pollution, economic development and international trade, and taxation of multinational enterprises that may be operating in the state cannot be ignored as matters of "low politics" by the central foreign policy establishment with its traditional focus on matters of "high politics." Brian Hocking, as author of the work under review and as editor of the recently published series of essays in *Foreign Relations and Federal States* (1993), is making a useful and timely contribution to the discussion of this important issue.

Although the author introduces the study with a chapter focusing on the localization of foreign policy, the emphasis of the book under review is better served by reference to the subtitle, *Non-central Governments and Multilayered Diplomacy*, rather than to the title. That local governmental units have some role to play in the formulation and implementation of national foreign policy, especially in federal states is not a new idea. That there may be tensions between the non-central governmental units and the foreign policy elite or establishment that operates as the central level is also not a new point. Hocking's argument goes well beyond these basic points. The traditional literature, he argues, tends to see the non-central governmental units operating as distinct and independent units which have incompatible interests with the central governments and which attempt to stake out their own sphere of influence. This argument, Hocking asserts, is misleading and fails to appreciate the complexity of the relationships between central and non-central governmental units. Hocking's argument is that the non-central units should be seen as participants along with the central governments in a process of multilayered diplomacy. It is not the case that the two units proceed separately in their diplomatic negotiations; rather the units interact with each other at the same time they may be interacting with the external governmental unit. The interactions between the central and non-central governmental units are complex, influenced by constitutional constraints and opportunities, occasionally confrontational, and occasionally supportive, and tend to be issue specific.

In making his argument, the author focuses on the role of non-central governments in three federal states: Australia, Canada, and the United States. Hocking offers three studies of diplomatic negotiations to support his basic argument: the United States-Canada free trade negotiations, the United States-United Kingdom dispute regarding the unitary taxation policies of several of the American states, especially California, and the United States-Canada negotiations over acid rain. These three diplomatic disputes offer insights into the role of non-central governments in three agenda areas that are important to both the central governments and the non-central units: trade, taxation, and environmental protection. Of the three studies, the discussion of the United States-Canada free trade negotiations is the most fully developed in detail and in analysis.

In his general overview and conceptual introduction as well as his general review of managing multilayered diplomacy, which provide very useful insights, the author discusses briefly other issue areas and draws upon examples from Australia and New Zealand as well. Unfortunately the author's discussion and analysis in these sections is uneven and occasionally frustrating. Specific examples drawn from one state to illustrate a particular relationship between central and non-central units are discussed but comparable questions that might be asked or illustrations that might be provided about the other states in his research are not pursued. Thus the reader is left wondering whether the relationship is state specific or whether it is a more generalizable one. In this sense one wishes that Hocking had been deliberately comparative in his approach.

Notwithstanding instances of occasional frustration, the reader will discover that Hocking sets forth a number of insightful and provocative prospositions about the nature of the relationships between central and non-central governments dealing with foreign policy matters and how those relationships are managed. These propositions offer fruitful opportunities for future research for students of comparative foreign policy as well as federalism.

University of Maryland DON C. PIPER

Morality, Prudence, and Nuclear Weapons. By Steven P. Lee. New York: Cambridge University Press, 1993. 418p. $64.95.

As strategically sophisticated as Robert Jervis, as morally subtle as Michael Walzer, as tautly logical as Thomas Schelling, as relentlessly tough-minded as Colin Gray, as imaginatively far-sighted as Jonathan Schell, and as calmly sensible as Lawrence Freedman, Steven P. Lee has produced a spectacular synthesis of the military and ethical considerations relevant to decisions about nuclear weapons in the wake of the Cold War. Strictly as an analysis of the purely "realist" considerations affecting deterrence and stability, or strictly as an analysis of the purely normative principles constraining strategic options, Lee's unfailingly lucid and scrupulously fair dissection of every major option would be invaluable. Yet, far from being either exclusively strategic or exclusively ethical, this remarkable study has genuinely interlocked morality and security (what the title somewhat archaically calls "prudence"). Constantly, moralists are confronted with the implications for stability and the likelihood of war of implementing their favored principles, and strategists are forced to unmask the moral reality of their favored options for preventing, limiting, and ending wars. This is, within one cover, the two best books I have read about nuclear weapons, the best one on the strategic logic and the best one on the ethical logic. The author has transformed both his subjects by restoring their unity.

The central argument has the deceptively simple thesis that morality and prudence—what is right and what is safe—must both be respected, not through some simple-minded compromise between the two, but each independently. Yet if morality requires policies that would likely be suicidal, while safety requires policies that are barbaric, we face an intolerable dilemma. Lee captures the dual necessity that morality does constrain self-interest significantly but does not mandate its surrender with his "principle of tolerable divergence" (p. 21). The basic question about nuclear weapons becomes, then, whether they have created an intolerable divergence between prudence and morality that can be remedied only by radical revision of either deterrent postures or ethical standards.

One of Lee's unusual contributions is to insist that the question cannot be answered by thinking vaguely about counterforce targeting, countercity targeting, and nuclear disarmament, as almost all other normative theorists have done until now. His subtle exploration of the rich and critically different varieties within each general option defies non-caricaturing summary, but one pinnacle of Lee's argument is a brilliant demonstration (p. 286–91) of how the stability/instability "paradox," used valuably by Glenn Snyder and others, inadvertently masks the risk of accidental nuclear war. Lee's theoretical innovation in response to the inadequacy he shows in the stability/instability dilemma is the suggestion that we think instead in terms of three, not two, kinds of instability, treating risks of accidental war separately from both intentional conventional attack and intentional nuclear attack. This turns out to be especially apt in light of the simultaneous publication with Lee's more conceptual work of Bruce G. Blair's *The Logic of Accidental Nuclear War* (1993) and Scott D. Sagan's *The Limits of Safety* (1993). Lee uses this original conception of a triangle of instabilities to formulate a perspicuous prudential trilemma among minimum deterrence (of nuclear attack only), extended deterrence (of conventional as well as nuclear attack), and counterforce deterrence (modest and ambitious), showing that each of the three fundamental strategic options respectively solves a different one of the three stability problems but fails to deal with either one of the other two. And Lee argues powerfully that this is necessarily, not contingently, the case: just as the stability/instability "paradox" was said to be insoluble, the stability trilemma truly is insoluble. No form of nuclear deterrence can possibly "work," on an adequate non-dichotomous understanding of what it means for deterrence to "work."

Does this imply, then, that nuclear disarmament, unilateral or multilateral, is called for? In a fascinating final argument about policy, which employs Jonathan Schell's insight that nuclear disarmament is unavoidably still a species of minimum deterrence with the number of warheads set at zero, Lee argues that immediate nuclear disarmament would create the same instabilities that all forms of minimum deterrence create, unless nuclear weapons were first delegitimated to such an extent that they lost their prudential value in solving the other two stability problems that extended and counterforce deterrence now solve better than minimum deterrence does. The moral imperative, then, is first to delegitimate and only then to disarm. To delegitimate we must practice the opposite of the foolish forgetfulness Henry Kissinger advised and always remember with crystal-ball clarity the horror that nuclear weapons will otherwise always hold in wait, if not for now, then for later. Meanwhile, the best policy is minimum deterrence, and, as Lee shows through insightful pair-wise comparisons, neither the extended deterrence favored by leading political scientists nor the nuclear disarmament favored by leading philosophers. It will be prudent to abandon only nuclear weapons that have first been delegitimated, but morality now requires we must move urgently to avoid continuing to rely upon them. We must actively contribute to changing the norms of international relations, and what nations can expect from each other, in order to be able to change our own dangerous practice of deterrence safely.

The one serious lack in this magnificent piece of interdisciplinary scholarship is the absence of even a select bibliography, which Cambridge could easily remedy in the yet-to-be-issued paperback.

Cornell University HENRY SHUE

Beyond Charity: International Cooperation and the Global Refugee Crisis. By Gil Loescher. New York: Oxford University Press, 1993. 260p. $35.00.

Very few persons know more about refugee affairs than Gil Loescher of Notre Dame University. Loescher has now put in one volume most of the basics, and some new ideas, about this perplexing and important subject. Writing for the Twentieth Century Fund, he has provided ample background to such contemporary developments as the exodus of over two million persons from Rwanda during 1994.

This is an essential book for scholars, students, and libraries. Perhaps its only major detraction is that specialists will have to wade through some well known points in order to get to the new ideas. Loescher acknowledges (pp. ix–x) that the book is meant to appeal both to the experts and others. But if the specialists can skim the basics, the rest will prove rewarding. Theorists will find that Loescher is not interested in speculating about what refugee affairs means for "regime theory" or other debates about various forms of "realism" or "liberalism" as schools of thought for world politics. Nevertheless, ample material is here that could be applied to those debates concerning, inter alia, whether states dominate world politics or whether international organizations can act independently.

The fundamentals are stated in admirably clear prose and based on carefully researched facts. The number of conventional refugees (those protected under international treaty as having a well founded fear of individual persecution), war refugees, and internally displaced persons has mushroomed. The Office of the United Nations High Commissioner for Refugees (UNHCR) has not been able to keep pace with the demands on it for protection and assistance. The OECD countries (24 industrialized democracies form the Organization for Economic Cooperation and Development) are increasingly shutting their doors in the face of increased migration of asylum seekers, even though the numbers they face (a 700% increase from the early 1980s to 1991) are much smaller than the number of refugees hosted by the developing countries. "In Malawi . . . where the GNP per capita is only $170, one in every ten persons is a refugee from Mozambique. This is the equivalent of the United States . . . suddenly admitting over 25 million Central Americans—the entire population of the region" (p. 8).

Given this situation, Loescher argues that those dealing with refugees (and with persons in a refugee-like situation) must approach the problem from a political, and not strictly humanitarian, standpoint. He argues that rather than building barriers to newcomers, OECD states should integrate refugee policy with other policies pertaining to human rights and also development (see p. 127). This means that OECD states should deal with the human rights problems that create refugees, and should link trade and other policies designed to produce economic growth to projected refugee solutions.

The situation is complex. Loescher notes that development policies may actually stimulate migration during certain stages of growth for certain regions, concluding that "any pro-development policy is unlikely to discourage mass departures from the South and the East in the near future" (p. 169). He continues to argue, however, for a long term development policy designed to keep citizens at home. He also takes a firm stand in favor of conditionality—the tying of development aid to human rights reforms in pursuit of pluralism and humane governance.

In his final chapter Loescher lays out an action plan for the future. While commendable in terms of morality and reason, much of this vision is no doubt beyond the reach of most states in the near future. Given the reluctance of OECD states to exercise leadership and bear the costs of paying serious attention to human rights in places like Bosnia or Rwanda, it is unlikely that they will, for example, amend the United Nations Charter to permit binding decisions by the Security Council for human rights rather than for security affairs (see p. 197). In fact, so far this has proven unnecessary, since the Council reinterprets human rights issues to mean security issues when the political will is there to take a binding decision. The primary problem is not lack of legal authority but lack of political will. Moreover, the U.S. in particular has numerous reasons of self-interest for avoiding Charter revision, fearing, inter alia, the results of many Southern votes in any such process.

Other ideas are stimulating, even correct, but ahead of the times politically. It is true that the UN Secretary-General "needs to have at hand a permanent standby military capacity" (p. 187). But there is a slight problem in that the Pentagon has rejected this idea. We should indeed move away from a situation in which the Conference on Security and Cooperation in Europe "lacks teeth to implement its decisions" (p. 191). But neither the 54 states of the CSCE nor the 16 states of NATO have found a sure and systematic way to do this in Bosnia and elsewhere. It would certainly be well for the exercise of state sovereignty to be predicated on responsible and humane behavior (p. 183). The international community may be stumbling toward recognition of this principle. But we are not there yet, as authoritarian rulers know from China to Iran.

The visionary aspects of Loescher's final chapter notwithstanding, this is a fine book, at once informative and creative. If combined with other recent material, such as the relatively new annual report on refugees by the UNHCR, or an earlier volume edited by Loescher and Laila Monahan, *Refugees and International Relations* (Oxford 1989), a reader can be well informed about this major and growing issue of world politics. The primary impediment to solution of the myriad problems is just one small matter: adequate political will by the OECD states that provide most of the material resources, and most of the potential places of resettlement, for refugees.

University of Nebraska, Lincoln DAVID P. FORSYTHE

Decision and Interaction in Crisis: A Model of International Crisis Behavior. By Ben D. Mor. Westport: Praeger, 1993. 179p. $49.95.

Contemporary scholarship in political science on the causes of international crises and war is deeply divided by alternative approaches to both theory building and empirical analysis. Ben Mor attempts to build bridges across the typical divides in the field by developing a formal game-theoretic model of crisis initiation and outcomes which allows for the influence of psychological variables. Furthermore, he then empirically tests the formal model in a case study of decisions by Israeli and Egyptian leaders in the crisis which escalated into the Six Day War of 1967. The book has much to offer to both scholars with a general interest in theories of crisis initiation and escalation as well as those with a more specific interest in the historical debate about the causes of the 1967 War.

Mor's premise is that powerful theories of crisis behavior require the integration of insights from both rational choice game-theoretic approaches as well as the cognitive psychological literature which focuses on the limits to rationality. In particular, Mor argues that one fruitful approach to theoretical synthesis is to incorporate into game-theoretic models psychological variables which allow for the preferences of decision makers to change within a crisis. As a result, Mor argues that more dynamic game-theoretic models of crisis behavior can be developed.

The strength of the book lies in the lucid and very accessible presentation of the game-theoretic model (chapters two and three). Mor clearly presents a typology of different crisis player types and then discusses the logical patterns of strategic behavior produced by alternative combinations of player types interacting with one another. From this discussion a range of intuitive as well as more nuanced hypotheses about the role that misperception, resolve, military strength, and deception play in crisis initiation and escalation is presented. The case study (chapters four and five) is also well done with the empirical analysis driven by the testable propositions derived from the model. Mor uses the model to make a very interesting argument that much of President Nasser's behavior can be explained in rational choice terms in which Nasser was trying to use deception to achieve the best possible outcome short of war for his country (himself). In the end, Mor argues that Nasser's strategy of deception was too successful since when Nasser stopped escalatory actions and attempted to resolve the crisis short of war, the Israeli leadership was firmly convinced of his resolve to use force and to not compromise and therefore launched what they believed was a necessary preemptive strike.

The major shortcoming of the book is Mor's failure to actually incorporate in a generalizable and systematic fashion psychological variables into his game-theoretic model. This is without question a formidable task but Mor posits this as one of the principal contributions of his book and thus it is imperative that he be judged on those grounds. The weaknesses are interrelated and twofold. First, Mor contends that psychological variables are incorporated into his model when he discusses the different components of a crisis player's preferences. It is not at all clear, however, in his discussion (chapter two) how psychological theories or insights provide the foundation for the formation of different attitudes or beliefs such as "peace with victory" vs. "peace without victory" or "make the opponent pay" vs. "cut costs." Similarly, Mor does not develop a link between psychological variables and conditions under which we might expect decision makers to change their attitudes or beliefs.

Second, because there is no strong theoretical basis for predicting the preferences of players in the crisis model, it is difficult to explain and generalize about when and why preference changes occur within a crisis. Earlier in the book (chapter one), Mor criticized existing game-theoretic models for their inability to accommodate change in the basic preferences of players but ultimately Mor makes limited progress on this issue. This weakness is apparent in the case study of the Six Day War when Mor is unable to explain why Nasser seemed to have changed his attitude towards loss during the crisis. When such a change in attitude, however, is assumed to have taken place, then Mor is able to draw out the logical consequences of such a change in Nasser's preferences for expected crisis behavior and outcomes. The danger is that ad hoc explanations could often be introduced into the model to explain away apparent mispredictions by arguing that player's preferences must have changed which then produced a different set of expected behaviors.

Mor is sensitive to such problems and avoids such pitfalls in his own empirical analysis. Indeed, Mor is refreshingly straightforward in discussing the strengths and weaknesses of his own theoretical and empirical work. In sum, Mor has tackled a large and complex topic in the field of international politics. While he has not fully succeeded in resolving the theoretical problems that he had identified in the existing literature, he deserves credit for pinpointing the right problems, outlining in general form the types of answers we need to develop, and attempting to constructively build on the work of scholars from alternative theoretical approaches.

University of Michigan PAUL K. HUTH

Regime Theory and International Relations. Edited by Volker Rittberger. New York: Oxford University Press, 1993. 470p. $57.00.

The concept of international regimes, far from being the passing fad Susan Strange predicted in 1983, has led to vigorous research agendas in the United States and Germany. The purpose of this volume is to explore these research initiatives, comparing the work across continents and among theoretical traditions. Presented at a conference, critiqued, and revised, the papers examine different facets of regime theory. How is cooperation possible between states claiming sovereignty? The answer is found in the relationship between the idealist's neoliberal institutional emphasis on law and norms and the realist's power and interests.

Rittberger's introductory essay traces the differences between the American and German approach to the problem in terms of issue areas and preferred approaches to outcomes. Keohane's introduction provides an overview of the essential questions and findings in this transnational research agenda. Each offers an excellent synthetic overview, which both expands and updates Haggard and Simmon's literature review (1987).

The essays are arranged in three major sections. The first group places regime theory in broad theoretical traditions or examines a "piece" of the theoretical puzzle. In the former category, Hurrell sees regimes within the framework of international society, community, and cooperation, as distinct from international law; Kratochwil places regimes within the logic of *contracting* by identifying five distinct forms of contracts; and Kydd and Snidal plead for theoretical diversity in regime study by returning to the contributions of game theory as a way of providing analytical rigor and coherence. In the latter area, Haufler examines nongovernment organizations as parts of regimes often understudied in the literature. One important thread connecting these theoetical essays is recognition that domestic structures and processes are too often overlooked.

In the second group of essays, regime formation and transformation is the focus. These articles explore the dynamics within regimes, across regimes, and at different stages of the regime process. For example, Krasner's contribution focuses on the interplay between the sovereignty metaregime and specific human rights issues, while P. Haas's essay traces regime dynamics within the issue of the environment, with special emphasis on knowledge of the epistemic community. Two of the essays present findings from major ongoing projects, both asking similar questions: Young and Osherenko summarize their findings on regime formation, based on power-based, interest-based, and knowledge-based hypotheses, while Efinger, Mayer, and Schwarzer do the same for an elaborate set of studies in the Tübingen Project. In both articles, the specification of alternative hypotheses provide excellent material for students attempting empirical regime research using systematic comparative case studies. Equally useful are the generation of multicausal frameworks so necessary for explaining social phenomena.

Formation and change in international regimes (as many theorists in the first section assert) is intimately linked to domestic structures and processes. Jönsson makes the link by suggesting that researchers explore the cognitive world of decision makers to enrich the regime framework. And Zürn, as his title suggests, brings the second image (back) by identifying both foreign policy types and actor characteristics (interest profiles) that are regime-conducive. This excellent article offers some suggestive foreign policy types that can be linked to regime-conducive behavior and specific complex hypotheses, and utilizes the case studies of the Tübingen Project to produce innovative findings. Zürn and his colleagues have clearly provided a useful approach to linking domestic and foreign policy behavior with international regimes.

The third group of essays examine regime consequences: Do regimes matter? Previous researchers who have dealt with regime outcomes have puzzled over the methodological dilemma: How can you prove that X causes Y, that the presense of a regime makes any difference? Müller, like other scholars, focuses on regime compliance as one requirement of regime effectiveness. Regime norms are internalized within the domestic process.

More broadly, Biersteker wrestles with the methodological dilemma by proposing use of historical counterfactuals. He offers students of regimes a systematic approach to the problem: If there had been no global debt regime during the 1980s, how would have the outcomes been different? Breitmeier and Wolf utilize evaluative criteria—a standard of stable peace—to assess regime consequences in the area of the environment. Does the regime contribute to the advancement of civilization and to democratization domestically? Each author is cognizant of both the methodological problems presented and of the necessity to examine both domestic-level and system-level factors in assessing regime consequences.

Regimes analysis has become a part of the core of international relations. In the last outstanding chapter, Mayer, Rittberger, and Zürn suggest how knowledge about regimes has been advanced by the contributions in the volume. Most importantly, they place regime analysis within a broader literature: How can there be goverance without government? How can there be cooperation without anarchy? Both macro and micro approaches utilized in the volume have propelled this research ahead.

In short, the book is one of the first systematic overviews of regime theory and its empirical referents since the seminal volume of *International Organization* in 1982. The work shows how a primarily American research agenda has been developed and amplified by European scholars, offering some findings heretofore available only in German-language publications. Several of the articles will "stand alone" as pieces of integrative scholarship, assessing the current state-of-the-art in regime research for future generations of scholars (Rittberger–Keohane–Mayer, Rittberger, Zürn). Others provide conceptual breakthroughs to parts of the regime puzzle (Zürn, Haufler). Still others will stand as summaries of research work elsewhere (Kratochwil, Haas, Young–Osherenko). And several provide key and innovative methodological parts of the puzzle (Biersteker, Efinger–Mayer–Schwarzer). Regime theory is significantly advanced by the publication of this volume.

University of Kentucky KAREN A. MINGST

Grasping the Democratic Peace: Principles for a Post–Cold War World. By Bruce M. Russett. Princeton: Princeton University Press, 1993. 173p. $19.95.

Bruce Russett's laudable book summarizes, dissects, and expands our understanding of the disinclination shown by democracies to fight each other, a finding that has spawned a minor cottage industry of analytic studies. Russett's principal contributions include the very broad empirical domain to which he applies his research, as well as his close scrutiny of the respective role played, on the one hand, by the institutional constraints that limit a democratic government's freedom of action, and, on the other hand, by the normative and cultural consensus that underlies political democracy. At a more general level, the book provides a powerful challenge to core assumptions of political realism and neorealism.

Starting from the observation that democracies hardly ever fight each other (although they show no disinclination to fight nondemocracies or to skirmish with each other short of warfare), Russett dismisses a number of spurious explanations for this regularity. For example, it is true that democracies tend to share common international institutions, and this in itself may discourage fighting; but, as he points out, peaceful relations generally precede the creation of shared institutions. In Russett's view, two properties of democracy deserve particularly close scrutiny: its normative consensus and its institutional structure.

Democratic norms and culture cannot make these countries inherently more peaceful—else, why are democracies so willing to fight autocracies?—but they may well operate to decrease bellicosity *among* democracies.

The normative understanding that promotes peaceful resolution of societal conflicts within democracy may be generalized to relations between nations espousing similar democratic beliefs. Accordingly, it may not be considered legitimate or necessary to resort to violence against other democracies, nor is aggressiveness from them feared (making preemptive attacks unnecessary). The same reasoning does not apply to relations between democracies and autocracies. At the same time, structural and institutional constraints stand to play a role, since democracies are constrained from going to war by the need to ensure the support of public and a consensus between branches of government (although this may make them somewhat more vulnerable to aggression by autocratic states).

Using a continuous (though rather complex) measure designed to assess the degree of a country's adherence to democracy and controlling for a variety of possible confounding factors (e.g., wealth), Russett examines the respective impact of measures of institutional constraints and normative restraints on war-proneness of pairs of nations during the past half-century. He concludes, as did previous studies that used a binary (democracy–nondemocracy) measure of political system, that while democracies are not less belligerent in general, they are certainly more peaceful in their relations with each other. In addition, his analysis suggests that democratic norms provide a more powerful inhibition to warfare among democracies than do institutional constraints (which, nevertheless, remain influential).

One of the book's original contributions resides in the empirical domains within which the relationship between democracy and peace is tested. A variant of the test is applied to the system of Greek city–states of the late fifth century B.C., though the results prove somewhat ambiguous. On the one hand, city–states that could, by *some* definition, be characterized as democracies did fight each other less often than those who probably or certainly were not democracies. On the other hand, those that were most unambiguously democracies did fight more often than predicted. Still, this finding might be distorted by the fact that all of the wars between clear democracies were part of the Athenian campaign against Syracuse, not completely independent events. (Also, the democracies were disproportionately naval powers, with an ability to project power and, hence, to fight.)

A variant of the test is also applied to nonindustrial societies, that is, populations that contiguously inhabit a territory, speak a common language and contain political units of various levels of aggregation (e.g., bands, villages, districts). Controlling for size of political unit and the effect of insularity, Russett finds that political units characterized by a high degree of political participation (a surrogate measure for democracy in a preindustrial society) are unlikely to fight each other. Accordingly, the relation between democracy and peace seems robust: it holds when a number of confounding factors are controlled for, and it applies to a wide variety of empirical domains.

One could quibble about certain technical details of analysis. For example, it may sometimes be hazardous to infer the power of political norms from a country's political stability and its lack of political violence (the Soviet Union displayed both qualities over numerous decades). Nevertheless, the book combines rigor and relevance, maturity and originality; it takes the democracy–peace link further than it has yet been taken and draws thoughtful policy implications from empirical research. Perhaps most importantly, it suggests that the political realist's Hobbesian conception of world politics may be decreasingly relevant in a world of democracies; and it urges us to take more seriously than political realism ever could the proposition that "the policies of states in international relations, and their peoples' support for those policies, derive in large part from their fundamental values and images."

University of California, Davis MIROSLAV NINCIC

The Limits of Safety: Organizations, Accidents, and Nuclear Weapons. By Scott D. Sagan. Princeton: Princeton University Press, 1993. 286p. $29.95.

This book is a significant contribution to three fields: international security studies, organization theory, and risk analysis.

Grounded in original research in U.S. national security archives, *Limits* reveals a disturbing history of near-catastrophes in the handling of nuclear missiles and bombers. During a period of high tension when pilots had been told there would be no tests, for example, a bear climbing a military-base fence in Minnesota touched off a "nuclear war has begun" alarm in Wisconsin, and only an officer in a jeep flashing his lights on the runway stopped nuclear-armed aircraft from taking off toward the Soviet Union. During the Cuban missile crisis, radar stations mistakenly showed practice tapes of a Soviet missile attack just at the time a satellite came over the horizon in the expected trajectory, leading to a report that Tampa had been destroyed. Alcohol- and drug-abusing nuclear weapons guards engaged in suicidal and homicidal behavior. Accident, curiosity, and bravado led American pilots to arm nuclear bombs and otherwise violate standard procedures. And virtually all the problems were either covered up or failed to serve as a source of organizational learning. The book moved me to reappraise the relative proportions of luck and strategy that went into the Cold War's absence of accidental nuclear detonations; I predict most readers will join me in that reevaluation.

The historical incidents are interpreted through lenses provided by organization theory. *Limits* formalizes, extends, and tests the comparative usefulness of two theoretical traditions: (1) Charles Perrow's normal accidents theory, holding that system failure eventually is inevitable in tightly coupled and complexly interactive organizations, such as the U.S. missile command; and (2) "High reliability theory," Sagan's own interpretation of research by La Porte/Rocklin/Roberts, Wildavsky, and Morone/Woodhouse, who seek in disparate ways to understand how organizations protect against catastrophic outcomes in the face of high uncertainty.

Sagan argues that normal accidents theory is the more illuminating theoretical tradition for his cases, since—time after time—redundancies and other built-in protections are overridden by organizational actors pursuing goals other than minimizing American citizens' risks from accidental nuclear discharges. SAC has a stake in keeping bombers flying, so it uses a bomber as a redundant warning system despite the possibility it can crash on a radar; a pilot wants to protect his career, so he covers up a potentially catastrophic error; a commander wants to avoid criticism, so he blames problems on subordinates rather than on shortcomings in policy.

Does the argument stand up to scrutiny? For the most part, yes; one comes away from *Limits* with a profound appreciation of the roles of organizational routines and actors' interests in shaping and misshaping the implementation of nuclear weapons safeguards. Extrapolation to other hazardous technologies is but a small, and justified, step—as evidenced by the *Exxon Valdez* and Bhopal accidents, among others correctly attributable to organizational dysfunction, and not merely to individual human error.

If the book has a shortcoming, it is that its theoretical treatment could be framed more even handedly. *Limits* is marvelous in depicting organizational forces leading to the breakdown of safety procedures, and the picture thereby painted is chilling. But it is perhaps equally important to ask, "Given tens of thousands of nuclear warheads, controlled by flawed humans who were supposed to respond almost instantly to ambiguous signals of Soviet threat, how did SAC and the other parts of the nuclear missile command manage to get through the Cold War without an accidental or erroneous detonation?"

Part of the answer is revealed by La Porte et al.'s studies of emergency flight deck operations on aircraft carriers, management of multi-state electricity grids, and other high-reliability organizations. These scholars have identified social-organizational requisites for near-perfect performance, including a thoroughgoing commitment to safety by top executives in the organization, intense training and socialization, and redundant backup systems. Neo-incrementalists Morone and Woodhouse find that complex, risky endeavors succeed in direct proportion to the extent there is diversity of participation in decision making so that fewer considerations are overlooked, flexibility in policy trials to enable error correction, redundancy and other means of mitigating the consequences of the inevitable errors, and vigilant monitoring of policy trials. Most of these decision strategies are to be found in the U.S. missile command, and arguably go far toward explaining the extent of its success in preventing accidental detonations.

Still, Sagan is right to underline the limitations of such high-reliability strategies. *Limits* causes us to focus more attention on conflicts of interest in organizations, even in those managing nuclear weapons and other hazardous technologies. Of redundancy, Sagan argues that at times it actually makes matters worse, inadvertently causing the very problems it was designed to prevent: SAC's redundant B-52 *did* crash next to the radar station in Thule, Greenland, for example, almost providing simultaneous blinding of two information sources and thereby increasing Air Force monitors' confidence that a Soviet attack was in progress.

Of flexibility, Sagan points out that partisans within one organization will not necessarily find it rewarding to pursue strategies that might be sensible from some Olympian viewpoint. On socialization, Sagan argues that strong organizational control can encourage excessive loyalty and coverup to protect the reputation of the organization. These and other arguments move the normal-accidents approach to organizational reliability a good bit beyond Perrow's largely structural views, offering a richer organizational politics interpretation.

I feel confident that my fellow "high reliability theorists" would join Sagan in expecting organizational politics (and the egregious shortcomings of contemporary democratic politics) often to interfere markedly with learning from experience and other facets of organizational reliability. In the case of nuclear weapons, moreover, everyone can agree that nearly perfect is not good enough. How, then, to do better?

Normal accidents theory recommends making an organization less tightly coupled and less complexly interactive, so that breakdowns in one part of an organization or activity do not ramify into larger and more catastrophic problems. One of Sagan's illustrations of this is to move warheads away from missiles in the post-Cold War era, allowing more time to evaluate whether an alarm is real, and less opportunity for catastrophic accidents. (Bombs now have been removed from bombers during peacetime, following a fire in a nuclear-armed B-52).

Altogether, *Limits* is perhaps the outstanding contribution to date in a developing genre that is rejuvenating the organization-theoretic approach to national security studies. After a long hiatus when Halperin, Steinbruner, and Allison moved into administrative positions and when neorealists and rational actor theorists were at their apex, Sagan reinvigorates the use of intra-organizational loyalties, routines, and power relations as means of understanding inter-organizational behavior in national security realms. (Also see Sagan's organization-theoretic analysis of nuclear proliferation in *International Security*.)

Sagan's conceptual analysis advances as well our theoretical ideas about organizational reliability under conditions of high uncertainty and potentially catastrophic risk. The book is sure to stimulate scholars from several traditions to sharpen their arguments to take account of Sagan's criticisms and insights. *Limits* also will encourage risk analysis scholars to take organizational politics into better account.

Finally, for organization theory more generally, Sagan joins Perrow, La Porte et al., and a handful of others in forging a normative wing of the field. These scholars are concerned with developing theories about how organizations *do* work in order to understand how organizations and their networks *can* work more reliably, more fairly, and otherwise "better." As a stimulus to further development of *relevant* organization theory, Sagan's contribution is most welcome.

Rensselaer Polytechnic Institute E.J. WOODHOUSE

The Limits of State Autonomy: Societal Groups and Foreign Policy Formation. Edited by David Skidmore and Valerie M. Hudson. Boulder: Westview, 1993. 328p. $42.50 paper.

This book is the first product of an occasional series of edited volumes sponsored by the Foreign Policy Analysis Section of the International Studies Association. Despite a growing number of publications in recent years, the role of domestic factors in explaining foreign policy remains a relatively underdeveloped field, and this is even more true for the role of societal groups. As the editors argue convincingly (p. 4), and I agree wholeheartedly, theories of international relations which focus at the systemic level of analysis may complement but cannot substitute for models which specify the domestic determinants of foreign policy. Our knowledge in this area, however, leaves much to be desired. Any effort to fill this gap should therefore merit applause and our attention.

The papers in this book were selected from among those received in reply to an invitation sent out in 1990. The editors have certainly been successful in selecting a number of papers which show, as they put it the "diverse theoretical and empirical approaches while assuring that the volume as a whole addressed the role of societal groups in varying national contexts" (p. vii). At the same time, this volume makes no exception to the general rule that the shortcomings of this kind of volumes are often all too obvious, i.e., the absence of a general framework and a convincing rationale for the ways and theoretical reasons why the respective papers were chosen, as well as the absence of any effort to draw general conclusions and in particular, to provide an answer to the question: "where do we move from here?"

It is true, indeed, that in an introductory chapter the editors do try to bring some (theoretical) order in the research agenda and the diversity of materials in this volume. They do so largely by distinguishing between what they refer to as *statist, societal* and *transnational* approaches or types of state-society relationships. This typology is then used to provide the categories by which this volume is, somewhat artificially, organized. These distinctions, however, do not return as organizing principle in the other theoretical chapters (including an interesting effort to provide a framework for comparative studies to which I return below)—however interesting and ingenious they are in themselves—and neither are these theoretical chapters related to or informed by the empirical chapters. To the extent that the latter have theoretical ambitions they point in rather different directions. Apart from the introductory, theoretical chapter, the book contains two other efforts at theorizing. One, by Douglas van Belle, looks at the options and strategies which rational leaders and governments have in dealing with forms of domestic opposition to foreign policy. In order to stay in power they will have to defer in some ways to these domestic pressures. However elegant and intriguing the elaboration of this model of reaction is, it does treat "domestic response" to government actions as a general category, and therefore does not really help us to understand the conditions under which societal groups in particular have a specific impact on foreign policy.

In the most ambitious (and longest) chapter, by Valerie M. Hudson, Susan M. Sims and John C. Thomas, a detailed effort (including coding rules) is made to develop a framework of analysis for comparative studies of domestic factors in foreign policy making. It is argued, correctly, that one needs to bring together country experts' knowledge with a generalized, systematic treatment (p. 55). This, however, remains a daunting task. This chapter, by primarily listing and defining the variables concerned can only be considered a first step in the right direction. Theorizing on the assumed relationships between the variables needs to be carried much further. For this, we keep needing heuristic case studies.

The book contains a number of such studies, which, on the whole, make for stimulating reading, and which raise many intriguing questions and hypotheses, which, however, in most cases are only answered and confirmed in an ad hoc fashion. Moreover, some of the case studies deal with issues which have continued to evolve after the respective papers were finished. Due to the long production time of the book, this was often a considerable time ago. In the cases of South Africa, the Israel-Palestine conflict and the end of the Cold War, the reader gets the feeling of being left in mid air when the narrative stops.

Nevertheless, the case studies retain their use to the extent that they suggest theoretical implications. In the study of the struggle over sanctions against South Africa in the United States, by Elizabeth S. Rogers, it is the concept of domestic costs and benefits and the impact of American blacks on foreign policy. David Skidmore, in his study of the domestic struggle over arms control, in particular the SALT II treaty, explores why proponents of arms control failed to produce a counterweight to the massive campaign of Cold War hardliners, and suggests how his results could be generalized. In his case study of efforts to combat the international drug trade at the beginning of this century H. Richard Friman shows the usefulness of "historical" studies as well as emphasizes the ways in which governments resort to duplicity and deception in order to cope with the crosspressures of domestic groups and international counterparts and avoid unpalatable compromises. Tamar Hermann contributed an interesting explanation of the gradual emergence of an independent peace movement in Israel in spite of the prevailing "siege syndrome" characterizing the public mind in this country. Finally, the book contains two contributions on the role and relevance of transnational movements and forces, one, by Ellen Dorsey, which is more general and philosophical-theoretical (and not entirely convincing), the other, by David S. Meyer, which is more empirical and focuses on the peace and human rights movements in Europe and the United States, and their role in stopping the arms race and bringing about an end to the Cold War. Both fail, however, to deal convincingly with the many conceptual and methodological issues raised by the analysis of the impact of transnational forces on international relations and individual foreign policies. Among the latter, the problem of separating the influence of societal groups from other potential causal factors remains the most pressing one, even after reading this book.

The most informative (and, for students probably most useful) chapter is the one by Harald Muller and Thomas Risse-Kappen, "From the Outside In and from the Inside Out," which surveys the voluminous literature (including a number of European writings which tend to be less known among Americans) and argues the need for empirical research which takes both the international/systemic level and the domestic level into account in foreign policy analysis.

Leyden University, Netherlands PHILIP P. EVERTS

Feminist Theory and International Relations in a Postmodern Era. By Christine Sylvester. New York: Cambridge University Press, 1994. 265p. $54.95 cloth, $16.95 paper.

Where might feminist theory find ontological, epistemological, and political homes in the field of international relations during a postmodern era? This is a central question raised by Christine Sylvester's latest contribution to feminist theory debates. Posing this question enables Sylvester to explore how various feminist positions (feminist empiricism, feminist standpoint, feminist postmodernism and postmodern feminism) are applied to international relations. After a theoretical consideration of these feminist approaches (chapter 1), she rereads the three discipline-defining debates in international relations theory (idealism vs. realism, traditionalism vs. scientific behavioralism, and positivism vs. post-positivism) through them (chapters 2–4). In so doing, Sylvester highlights how international relations theory has excluded and continues to exclude feminist contributions while she effects a feminist repositioning of international relations theory. Read through Sylvester's compendium of feminist interventions, international relations theory is not simply supplemented by feminisms. It is transformed by them, as she illustrates with her analyses of how women's activities at the Greenham Common and in women's cooperatives in Harare, Zimbabwe, reinscribe international relations concepts of security and cooperation (chapter 5). These are but two illustrations of what Sylvester calls a "repainting of the canvases of IR" (chapter 6).

This repainting is attempted through a questioning of subjectivity, linked to epistemology/methodology and concern with political action. Sylvester stresses the necessity "of exploring the possibilities for identity hyphenations, metamorphoses, and mobilities" so that we may "consider various ways of knowing that help us to slip and slide around identity-constituting subjectivities in theory-useful ways" (p. 10). It is on these issues that *Feminist Theory and International Relations in a Postmodern Era* is the most theoretically ambitious. Making ontological/epistemological/political connections leads Sylvester to (1) propose two concepts—what she terms *homesteading* and *empathetic cooperation*—as the methodological basis of a postmodern feminist approach to IR; and (2) argue in favor of postmodern feminism and against feminist postmodernism as a guide to political action in a postmodern era.

For Sylvester, homesteading refers "to processes that reconfigure 'known' subject statuses . . . in ways that open up rather than fence in terrains of meaning, identity, and place" (p. 2). It is marked by the "double images of emancipation and exclusion" (p. 2), for as Sylvester employs it, homesteading becomes a site of potential emancipation while drawing attention to exclusions of gender, feminisms, (unproblematized) women, and (socially constructed) "women." Empathetic cooperation is configured as a method which does "not simply rehearse old inclusions and exclusions or reinforce old readings. Empathy leads to listening to the excluded, listening to their sense of the good, knowing that they will present a fractured and heavily contested discourse because they have been simultaneously inside and outside a master narrative. Cooperation comes in rescripting agendas to reflect the subjectivities that have been etched into the identities of empathetic listeners" (p. 165).

Homesteading and empathetic cooperation are best expressed by what Sylvester (following Nancy Hirschmann, *Rethinking Obligation* [1992]) labels postmodern feminism, not to be confused with feminist postmodernism. "The subtle point of difference between a feminist postmodern and postmodern feminist position would be that the latter would tolerate the politics of self-assertion on the way to a politics of empathetic cooperation" (p. 53). In other words, Sylvester argues that feminist postmodernism risks abandoning subjectivity—a necessary home for political action. She critiques some postmodernists by arguing that, "To cut ties to socially constituted sex at this time is to risk leaving certain subjects out of the future by virtue of ignoring any of their stories that revolve around gender experiences" (p. 182). She embraces postmodern feminism because she argues, "Only when ontology is affirmed can the politicized Self relax into many hyphenated identities" (p. 53).

Sylvester's theoretical discussion of the differences between feminist postmodernism and postmodern feminism in some respects occludes rather than sheds light on how these two feminisms theorize ontology. For example, Sylvester hints that for feminist postmodernists, "women" have no basis in material reality when she writes, "If gender is a home for some 'women,' then it makes no sense to refuse them that identity because, technically, it is not real" (p. 53). But feminist postmodernists do not deny the material reality of women, nor do they refuse gendered implications of women. Instead, they are attentive to how the representation of real women in language obscures or overwrites women's realities. In this sense, feminist postmodernists widen the scope of politics by insisting on the materiality of language. Political action for feminist postmodernists includes exposing and disabling discursive techniques of exclusion, domination, and erasure. While this involves an analysis of the textual construction of ontology, it hardly amounts to its denial.

The differing approaches of feminist postmodernism and postmodern feminism to questions of ontology is not, as Sylvester suggests, a matter of the first approach denying ontology while the second embraces it. Instead, their differences concerning ontology hinge upon their relationships to boundaries. Feminist postmodernists problematize boundaries; postmodern feminists, like standpoint feminists, affirm them. For postmodern feminists, what is within these boundaries may be multiple, hyphenated, and mobile, but the boundaries themselves are accorded a degree of acceptance. The effect of postmodern feminism is to construct a "home" for a stable and self-identical subjectivity, however plural that identity may be.

In its search for homes, Sylvester's account of postmodern feminism shares a nostalgia for a politics of place (William Connolly, "Democracy and Territoriality," [1991]) that Sylvester finds so contemptible about most of international relations theory. While postmodern feminism's nostalgia is rooted in a desire for feminist political action in a postmodern era, it paradoxically betrays a fear concerning "the replacement of truth [stable subjectivities] with politics [calling the boundaries which fix identity into question]" (my brackets; Wendy Brown, "Feminist Hesitations, Postmodern Exposures," *differences* 3, 1:63–84, 1991). What eludes Sylvester's attention is that feminist political desires can be satisfied from a postmodern feminist unbounded or nonplace, just as it can be from feminist empiricist, feminist standpoint, and postmodern feminist positions. One need not meld standpoint epistemology with postmodern sensitivities in order to enable feminist politics in a postmodern era. Indeed, this move returns political action to a radical form, as Sylvester herself concludes when she argues that political action for a postmodern feminist means a return to socialist politics (p. 63). Rather than affirm how feminist politics is carried out from various feminist positions and nonpositions, *Feminist Theory and International Relations in a Postmodern Era* ultimately reinscribes an arbitrary boundary between what is political and what is apolitical by (to use Sylvester's term) evacuating postmodernism ontology, epistemology, and politics out of postmodern feminism. For all of its important contributions to the study of international relations and feminist theory, this aspect of Sylvester's textual canvas repaints international relations theory in strokes that are all too familiar.

Purdue University CYNTHIA WEBER

International Migration and Security. Edited by Myron Weiner. Boulder: Westview, 1993. 333p. $55.00.

With the end of the Cold War, the concept of what constitutes national security has changed dramatically and acquired a new and complex significance. Considerations of national security have expanded from traditional military–political issues at the interstate level (e.g., protecting a country's borders or continued existence from external threats) to many new concerns, including refugee and migration flows.

Refugees and displaced people are high on the list of international concerns today not only because of their humanitarian importance but also because of their impact on peace and security. It is no longer sufficient to discuss the subject of forced migration within a narrow national context or as a strictly humanitarian problem requiring humanitarian solutions. Despite this recognition of the underlying political nature of refugee movements and of the strategic effects of forced migration, surprisingly little systematic research has been carried out to explain the interrelationships among foreign policy factors and international security concerns and population movements. A conference, "The Impact of International Migration on the Security and Stability of States," held at MIT's Center for International Studies in Cambridge in December 1991 resulted in what is the first edited book on the subject, *International Migration and Security*.

The 11 essays in *International Migration and Security* attempt to develop an analytical focus in order to link these previously disparate fields of international security and population movements. Myron Weiner's introductory essay provides an excellent overview of the variety of ways in which migrants and refugees are perceived as potential threats to the security of states. Part 1 analyzes the migration flows that make societies ethnically either more homogeneous or more diverse, and examine the consequences of these movements for the security and political stability of states. Rogers Brubaker details the changing relationship between the ethnic minorities in the successor states of the Soviet Union and their own newly independent homelands and argues convincingly that a key determinant of future population movements will be how the debate works out over whether people of migrant origin enjoy the same rights as indigenes. William Zimmerman looks at how the war among the successor Yugoslav republics are aimed at moving borders and peoples so as to create ethnically more homogeneous societies. Rosemarie Rogers describes how and why migration has come to be regarded by many Europeans as a threat to societal stability and argues that the fundamental issue for most West European countries remains one of how to integrate migrant populations initially admitted with the status of temporary residents. Gary Freeman examines the foreign policy and trade considerations behind Australia's decision to open migration to non-Europeans and then to opt for an explicit multicultural policy.

Part 2 explores the security implications of international migration for the Third World, where the majority of population movements occur. Papers by Myron Weiner on South Asia, Astri Suhrke on Southeast Asia, Karen Jacobsen and Steven Wilkinson on sub-Saharan Africa, and William Stanley on Central America examine the sociopolitical conditions that generate population movements, the consequences for sending and receiving states, and the policy responses. Weiner writes that until recently, throughout the Third World, borders either did not exist or were not clearly demarcated, and people moved freely without regard for national boundaries or legal conceptions of citizenship. Part 2 argues that with the mass exodus of peoples from many developing countries and the high costs these migrations impose on receiving countries, many Third World states, particularly those with precarious ethnic balances or those caught up in regional conflicts, are closing their borders and are more clearly defining their residents on the basis of citizenship.

Part 3 analyzes the political role of ethnic diasporas, the millions of people who now live in a country other than the one in which they were born yet retain loyalty to their places of birth. Gabriel Sheffer and Yossi Shain examine how these new transnationals are mobilized, what their specific roles are for sending and receiving countries, and what the implications are for the conduct of interstate relations.

An important point made by several of the contributors to this volume is that contrary to popular belief, population movements are often welcomed and can, in fact, strengthen both sending and receiving countries. Australia purposively expanded migration from Asia in order to attract educated and entrepreneurial migrants with ties to businesses in Asia and to increase trade from that region. In Central America, sending countries like El Salvador, Guatemala, and Nicaragua have benefited from significant remittance flows from their migrants in the United States, and these foreign-exchange earnings helped stabilize the economies of all three of these war-torn countries. In Africa, refugees have in some cases served to enhance the host country's security either through the incorporation of refugees into their armies or by the in-flow of international refugee assistance and aid in the form of foreign exchange or infrastructural benefits. Most past research on international migration and security has focused on the negative impact of migration on interstate relations. Analyses of the benefits of population movements and their contribution to security should be given more attention in future studies.

Although the regional breadth of *International Migration and Security* is impressive, some important regions are not covered. For example, the Middle East, North Africa, and East Asia (regions in which migration and societal stability are closely intertwined and becoming increasingly important) are not examined in this book. The Middle East, in particular, is a region where there have been recent mass expulsions of national minorities and migrant workers (e.g., to Yemen and to Jordan from Iraq, Kuwait, and Saudi Arabia) and where migrants have played a crucial role in the economies and, to a certain extent, the politics of the oil-producing states. Similarly, it would have been useful to have concluded the book with an integrative chapter examining the implications of the links between population movements and security for the operations and present capacities of international and regional security and humanitarian institutions and mechanisms.

This collection of papers, along with the contributions of a few other scholars, have begun to map out an important and previously neglected area of international relations research. What is required in the future is not only more comparative work examining the interrelationships between migration and societal security but also more imaginative inquiry into what sorts of policies, institutions, and coalitions are needed both to maximize the positive contributions that migrants make to national, regional, and international stability and to create international order in a world in which there will continue to be large-scale population movements in the years ahead.

University of Notre Dame GIL LOESCHER

Justice and the Genesis of War. By David A. Welch. New York: Cambridge University Press, 1993. 335p. $49.95.

The central claim of this book is that states want justice as well as power. Welch explores the role of what he terms the *justice motive,* defined as "the drive to correct a perceived discrepancy between entitlements and benefits" (p. 19), in the genesis of five wars over the last century-and-a-half. The author's aim is not to develop or test a general theory of war causation. Rather, it is to show that the justice motive has been an important—in some cases, decisive—influence leading to interstate war and, in so doing, to bolster the case that the justice motive plays an important role in international affairs generally. This inference leads the author to speculate, in the concluding chapter, on how the justice motive might be harnessed to the cause of constructing a peaceful world order.

The book's primary target is *political realism,* which, in the author's view, provides an incomplete, if not distorted, conception of human nature. Realism assumes that all states are motivated by self-preservation and self-aggrandizement, which can be fully captured by a singular cost–benefit calculus. Against this view, the author argues that policymakers respond not only to "aversion to loss" and "appetite for gain" but to a desire to see entitlements fulfilled or respected. The motive for justice is of a different character than the motive for self-aggrandizement, in the author's view, not least because "the mode of reasoning involved in the defense of one's entitlements differs fundamentally from the mode of reasoning involved in the pursuit of other goods: it tends to be categorical and deontological rather than utilitarian" (p. 21). Realism, in the author's view, fails to recognize the distinctive character of the justice motive (if it recognizes this motive at all) and thus offers an inadequate basis for understanding either the genesis of war or the possibilities for peace.

Welch assesses the contribution of the justice motive to the outbreak of five conflicts: the Crimean War, the Franco–Prussian War, World War I, World War II, and the Falklands/Malvinas War. Because these are Great Power wars, the author argues, realism would suggest that these conflicts are motivated primarily (if not solely) by power and material interest. As such, they constitute "crucial cases": if the justice motive is found to be influential in these episodes, realism is thrown into general doubt. In each case study, the author is concerned to characterize both the motive's *intensity* (relative weight vis-à-vis other motivations) and *agency* (role in triggering the conflict). The author carefully explains his methodology and forthrightly addresses a number of difficult questions regarding research design. He concludes that the justice motive played an important role in all but one of the five conflicts (the Franco–Prussian War). In the last of the wars surveyed—that pitting Argentina against Great Britain in 1982—the author finds that the justice motive alone conditioned and triggered the conflict.

Welch's characterization of realism as a theory of human motivation is unusual. Readers who prefer to see in realism a theory of international politics (rather than foreign policy) and an explanation of recurrent political patterns (rather than singular events) may be unmoved by the author's assault on the assumptions about human nature underlying such theory. This is particularly likely where such assumptions are defended in terms of their explanatory utility, rather than their truth, and where parsimony is given pride of place in the assessment of competing theories; for Welch's account of motivations necessarily implies a messier, more detailed, more historically sensitive theory of war than does the realist account he aims to correct. But such objections to Welch's work would be misplaced; for what is most valuable in his book is not his theory of war causation (which, in any case, is, as he points out, only partially developed) but his insistence that a more adequate understanding of human motivation is necessary if we are to understand fully the challenges of building a just and peaceful world order. In the last chapter, "Justice and Injustice in a Global Context," the author argues against universalistic formulations of international justice and in favor of a regime-based "patchwork" conception of justice capable of dealing with the actual claims made by states in concrete circumstances. These normative arguments are based on the empirical analyses of motivation developed in the first six chapters. The interplay of these chapters is meant by the author to break down the barriers to communication across the "empirical–normative gap" in political science: "Explanation and prescription need not remain forever in their present artificial isolation" (p. 218).

The book is clearly written, soundly organized, and judiciously argued. The case studies are balanced and concise. While one might disagree with the author's characterization of realism as a theory of motivation, the book's contribution to our understanding of state motivations is nonetheless valuable. Most important may very well be the author's move to bridge the gap between normative theory and empirical analysis in the study of war. Certainly, the question of what institutions might best promote a just world order requires more than the single chapter the author here can devote to its discussion. But too many empirical studies of war have ignored this critical issue. Toward its clarification, Welch's book is an important first step.

College of William and Mary DAVID DESSLER

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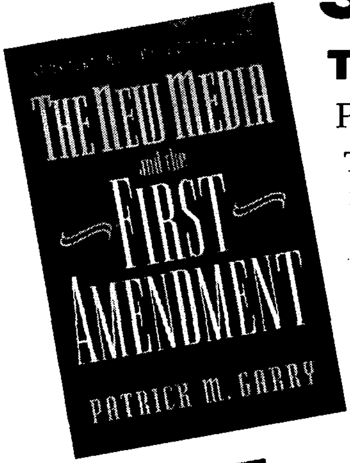

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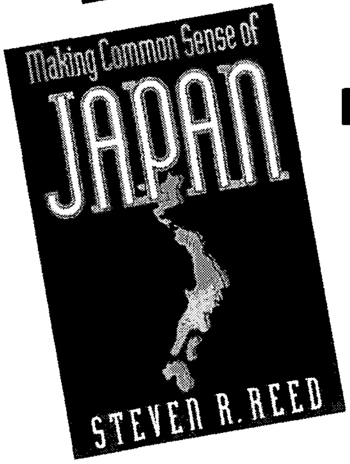

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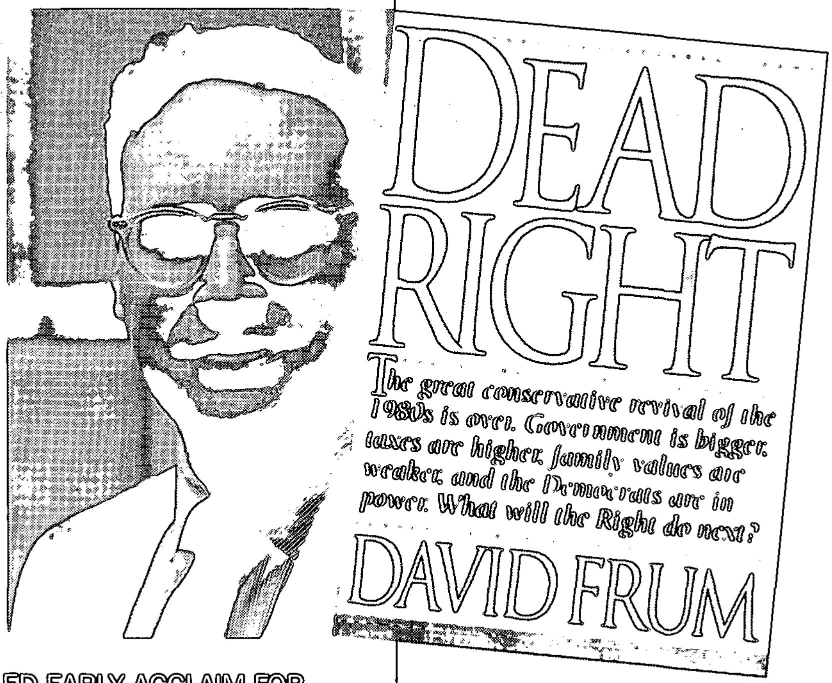
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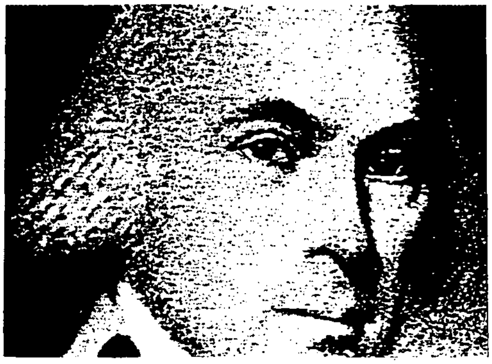

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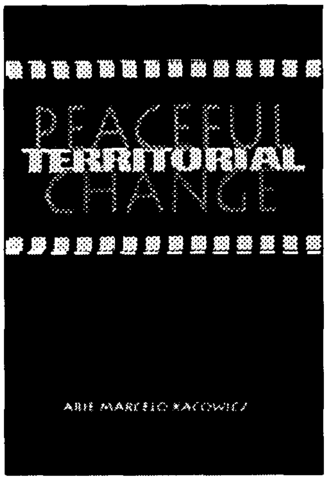

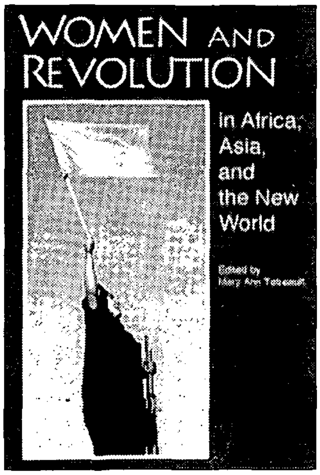

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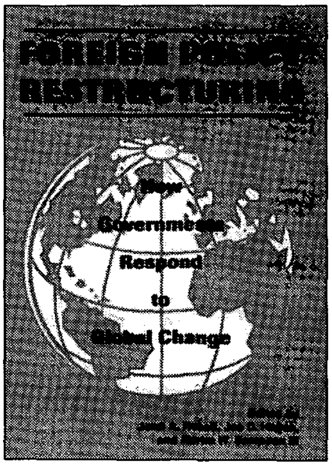

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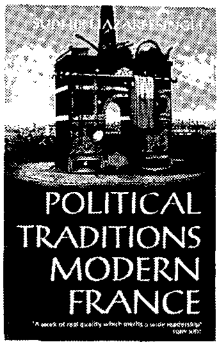

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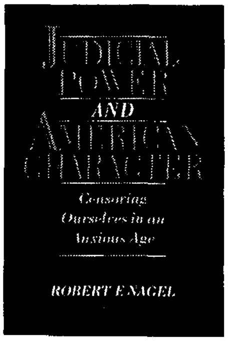
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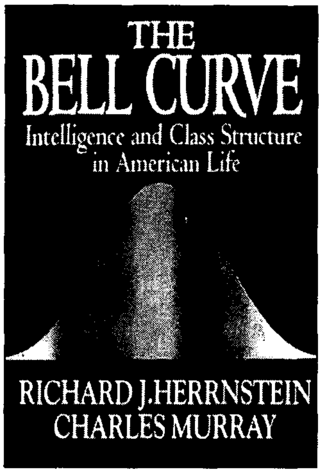

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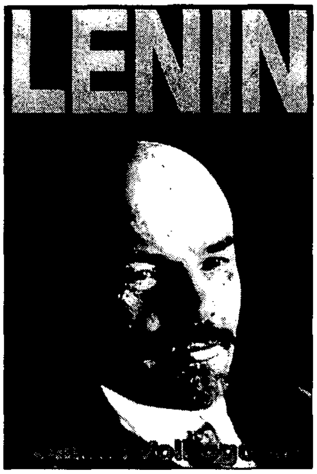

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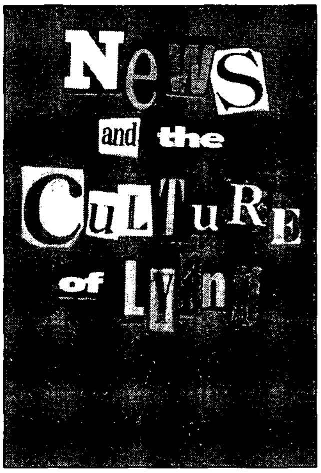

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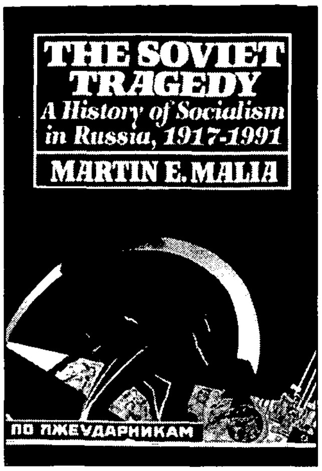

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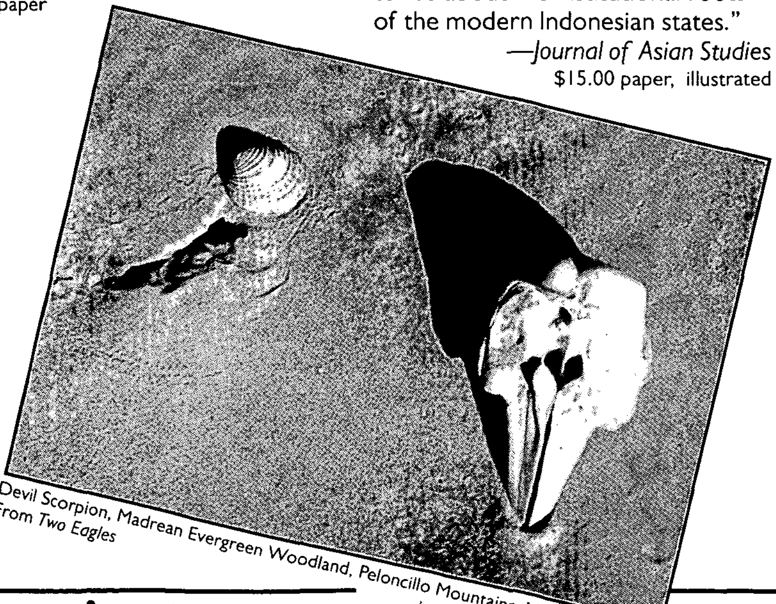

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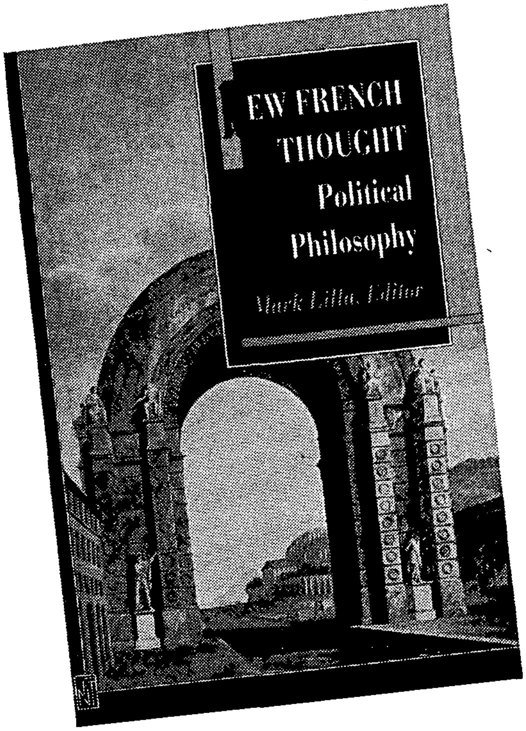

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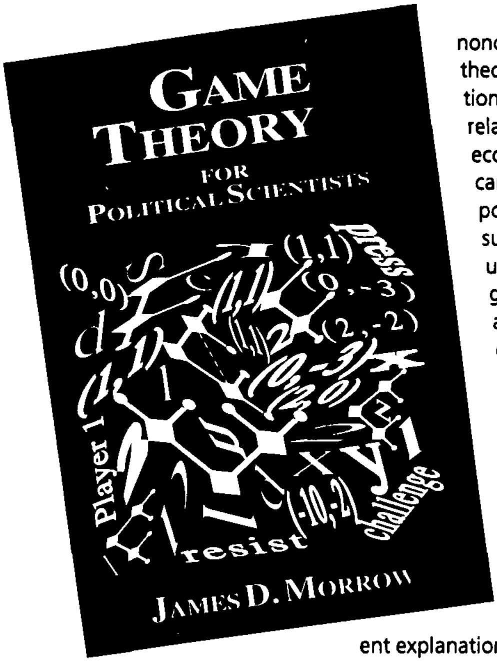

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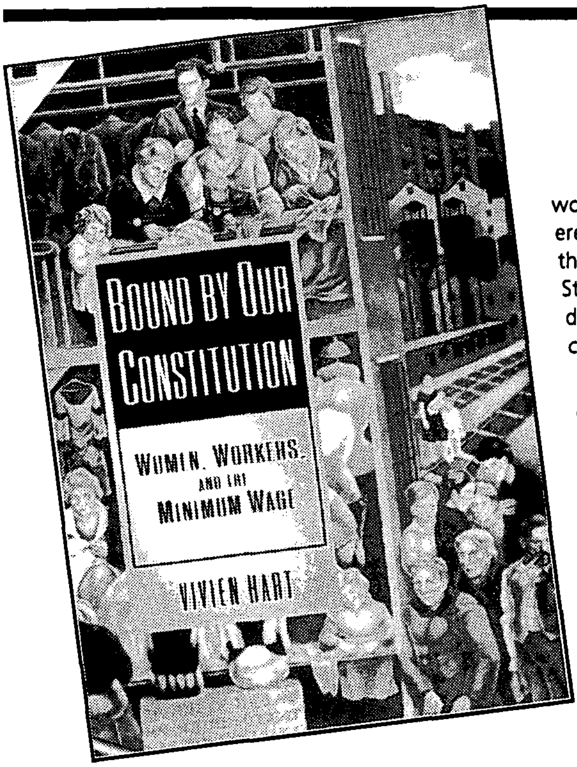
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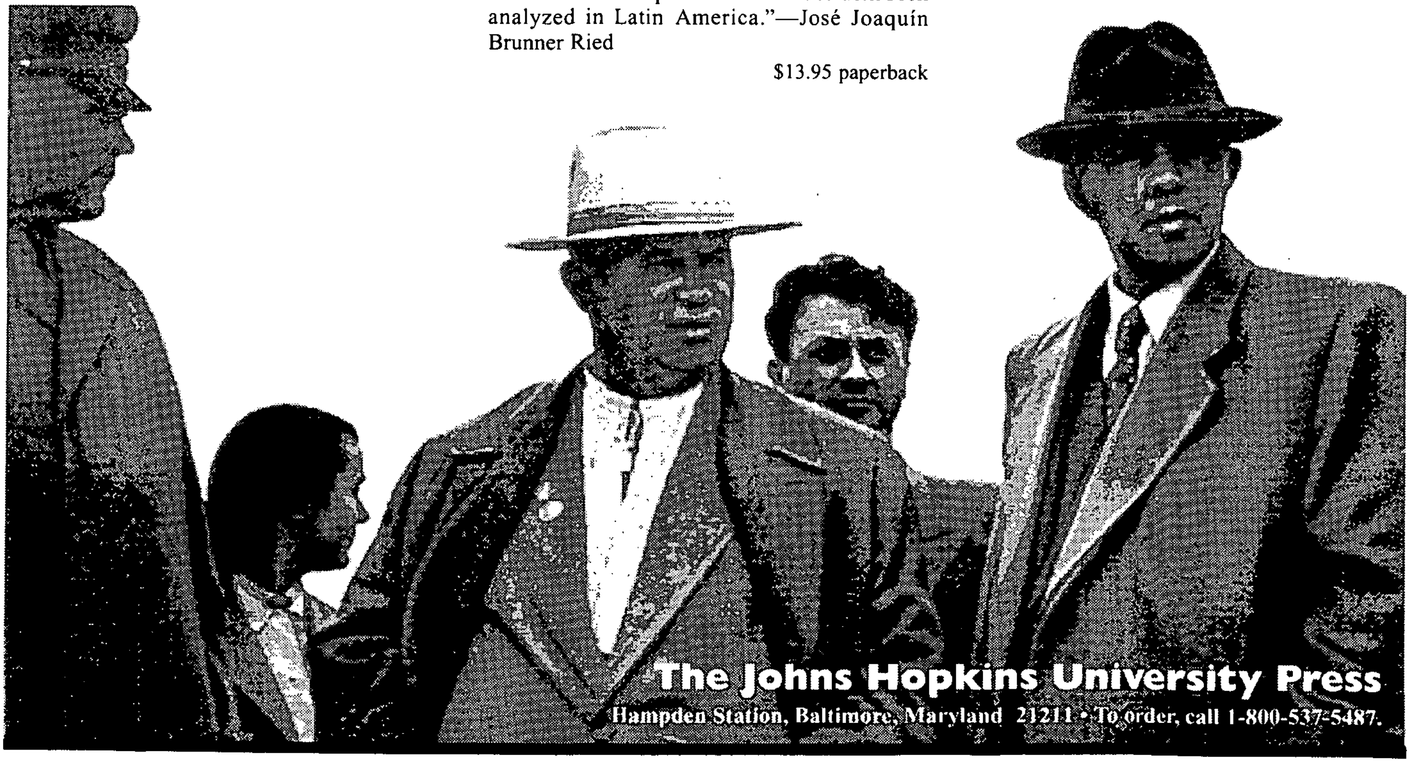

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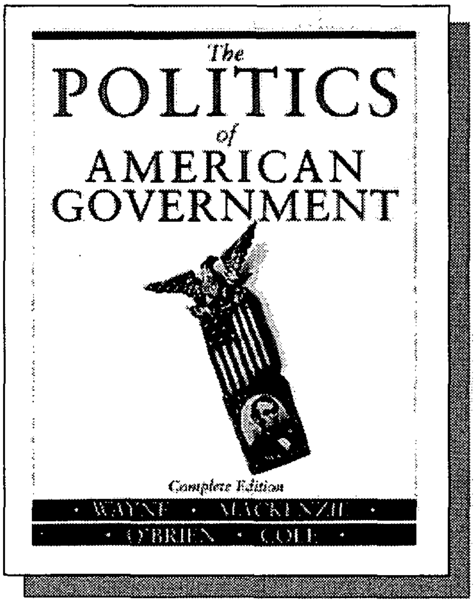

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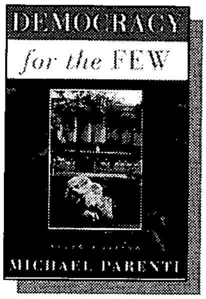

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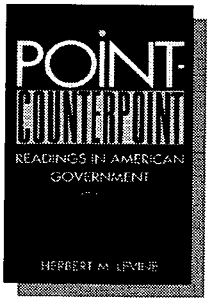

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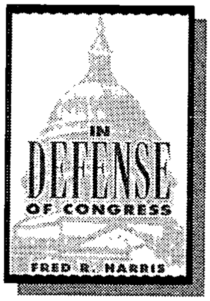

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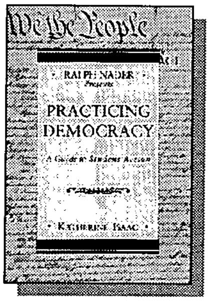

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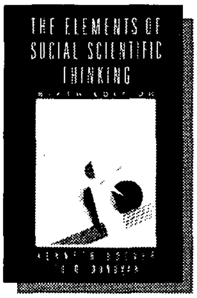

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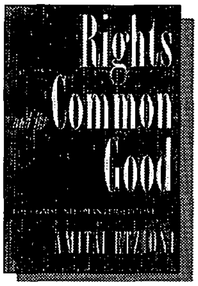

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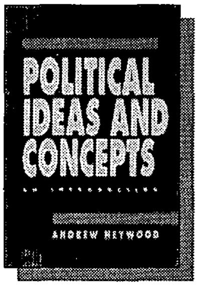

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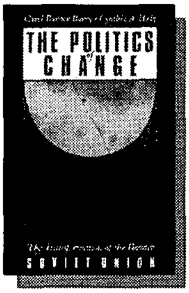

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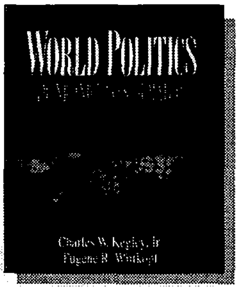

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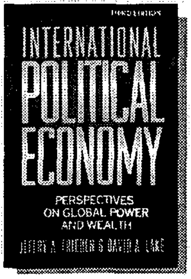

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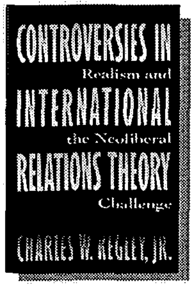

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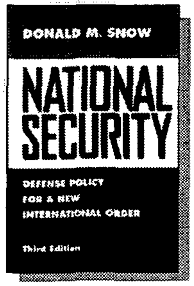

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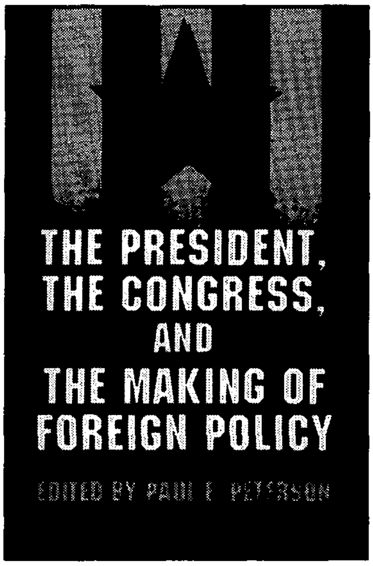

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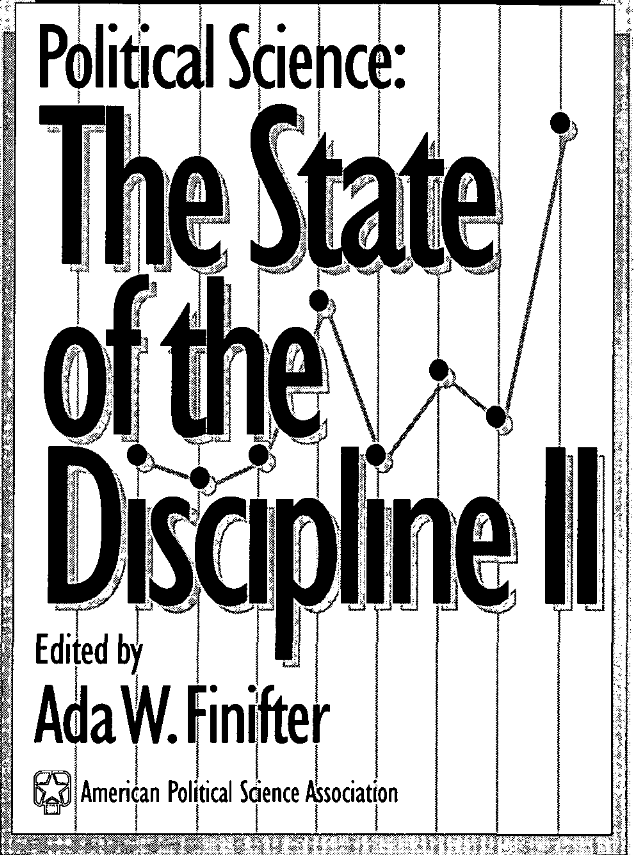

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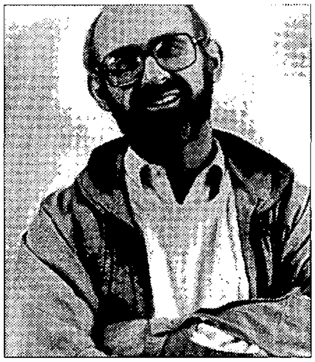

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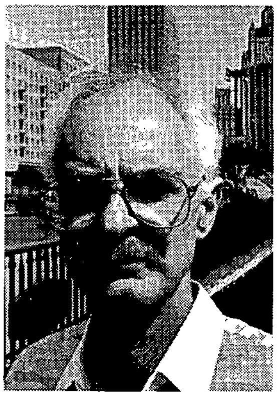

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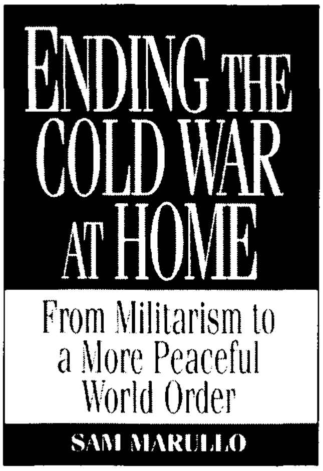
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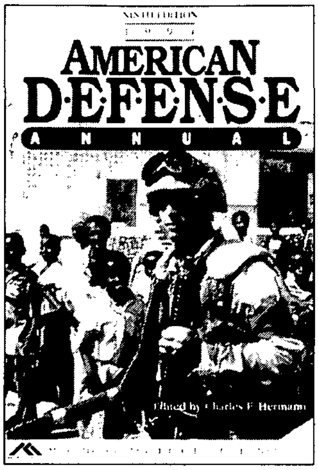

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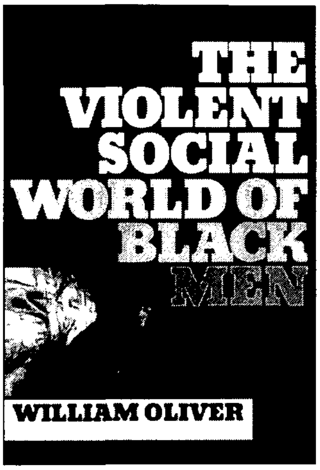

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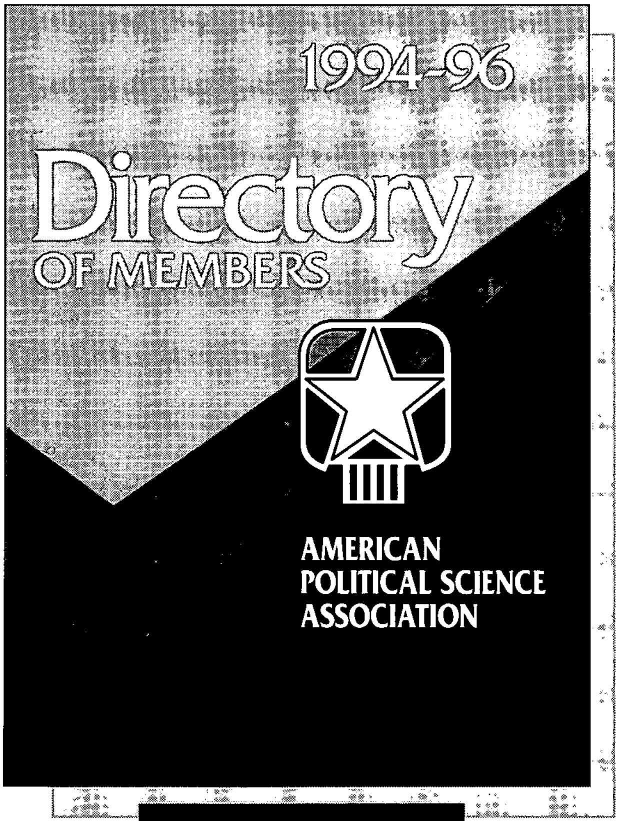

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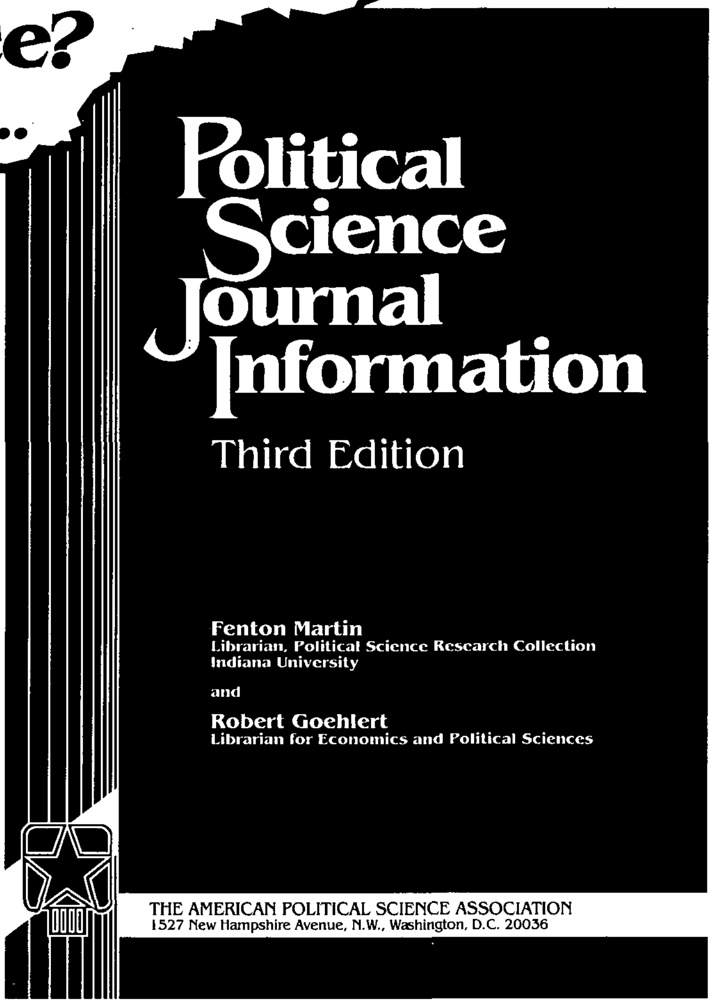

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